Constitutive or Regulative Principles?

The Kantian Legacy for Contemporary Philosophy of Science

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I, Jonathan Everett, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Abstract

Kant's philosophy of science is often taken to be straightforwardly refuted by the development of modern science and mathematics. I identify two ways in which key Kantian insights can be defended in contemporary physics: the first—associated with Michael Friedman—emphasises the role of constitutive principles in Kant's philosophy and the second—associated with Ernst Cassirer—emphasises the role of regulative principles. I argue that the regulative approach of Cassirer is the more promising.

I identify two challenges that a Kantian philosophy of science must meet in order to be deemed plausible: (CR) it must provide an account of the rationality of theory change and (CC) it must make sense of the central Kantian idea of constitutivity. I use these challenges to gauge the success of constitutive and regulative approaches throughout.

In §1 I introduce Friedman's constitutive approach. His answers to CR and CC are examined. I outline the role of philosophy in Friedman's answer to CR and stress the importance for Friedman of defending the syntheticity of the relativized a priori.

In §2 I detail the origins of constitutive and regulative principles in Kant's philosophy of science. It is emphasised that for Kant, both types of principle are essential to the possibility of science.

In §3 I introduce Cassirer's regulative approach. The regulative approach is defended from Friedman's objection that it cannot provide an account of the prospective rationality of theory change. Cassirer's understanding of the constitutive and regulative a priori are distinguished. Cassirer's structuralism is introduced.

In §4 I provide a case study of the role of the equivalence principle in the development of general relativity. A regulative Kantian answer to CR is defended.

In §5 I defend Cassirer's answer to CC as a plausible contemporary alternative to ontic structural realism.

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Introduction

Challenges for a contemporary Kantian philosophy of science

A. The relativized yet constitutive a priori

A central feature of recent attempts to revive a Kantian philosophy of science is the emphasis on the notion of a relativized, but still constitutive a priori. Michael Friedman's work (2001; 2008; 2010a; 2012) has been especially influential in defending this view, but it is also an important feature of the work of Ryckman (2005) and DiSalle (2006) and is a common thread that runs through the articles of Bitbol, Kerszberg and Petitot's (2009).

In Kant's philosophy constitutive principles governed the application of the faculty of understanding to the manifold of intuition. In effect they are rules for the construction of the phenomenal world. In Kant's analysis of Newtonian physics, the constitutive parts of the theory were taken to be Newton's three laws of motion and Euclidean geometry. Kant understood both Newton's laws and Euclidean geometry to be necessary rules for the construction of the empirical side of the theory, i.e., the law of gravitation. The laws of motion are necessary because they are derived by carrying the idea of matter as possessing moving force through the categories of relation. Euclidean geometry is necessary because it is the geometry of pure intuition.[1]

It is no longer plausible to understand science in precisely Kant's fashion, chiefly because Kant claims that his constitutive principles are necessary and true for all time. In the nineteenth and twentieth centuries developments in physics and mathematics entirely undermined this aspect of Kant's philosophy. The most damaging developments to Kant's philosophy of science come from physics: general relativity takes the geometry of space to be variable and a matter for empirical investigation rather than known a priori and quantum physics seems to cast doubt upon the constitutive role of Kant's understanding of the causal principle.

While some of the details of Kant's position must be abandoned, there remains a significant appeal to aspects of his philosophy. In particular the idea that in science

[1] I discuss Kant's philosophy of science in §2 and explain Kant's understanding of constitutivity in more depth there.

humans, to an extent, seek to make the world fit our own conceptual framework seems to have some merit. Examples of this abound in physics. Consider the dual nature of the photon in quantum physics: on occasions it must be treated as a wave, on others it is treated as a particle. There is an intuitive, broadly Kantian, way to understand this practice: what we refer to is not adequately captured by either the concept of a wave or the concept of a particle. It is likely that what we refer to as a photon fits into some altogether different category that humans have no concepts for. Indeed the very desire to treat bosons and leptons as *objects* seems to be a consequence of our imposing a conceptual framework developed in the realm of classical physics onto the quantum realm.[2] Even broad methodological demands in science—e.g. the demand for theoretic unity—should not be understood as revealing something about the world, but instead as revealing something about how humans must reason about the world.[3]

The Kantian approach, broadly construed, recommends analysing the conditions for the possibility of science. The idea is that by identifying the presuppositions of our scientific theories then we can have a clearer idea of the extent to which we should accept what our theories tell us about physical reality. This type of approach, I would suggest, has the potential to capture and account for the human contribution to scientific knowledge.

Two of the most prominent early philosophical accounts of the theory of relativity—Reichenbach's *The Theory of Relativity and A Priori Knowledge* (1965 [1920]) and Cassirer's *Einstein's Theory of Relativity* (1923 [1921])—took precisely this type of Kantian line: in particular, both sought to relativize Kant's constitutive a priori. Reichenbach, famously, expressed this central idea in the following fashion:

> Kant's concept of a priori has two different meanings. First, it means "necessarily true" or "true for all times," and secondly, "constituting the concept of object" (1965, p.48)

So, Reichenbach and Cassirer[4] both seek to retain some sense of constitutivity while dropping the claim that the constitutive a priori must be true for all time.

However it is not enough to simply drop the claim that constitutive principles have to be necessary from Kant's philosophy: this claim that the categories of the

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[2] I develop my account of how we should understand objects in §3. The basic problem with understanding bosons and leptons to be objects is that one cannot re-identify the position of a putative object because the position observable is a continuous function. Re-identifiability is a crucial aspect of our conception of objecthood. See (French, 2001) and (Mittelstaedt, 2009) for discussion of this feature of quantum physics.

[3] See (Morrison, 2000) for the most detailed analysis of this understanding of the role of unity in science.

[4] My claim here that Cassirer held on to some notion of the relativized a priori is controversial. Friedman (2000, p. 155ff)) argues that Cassirer has only an absolute and regulative conception of the a priori, so he would certainly reject my ascribing this view to Cassirer. However, I think that Cassirer's (1923) provides ample textual support for the idea that he defended a version of the relativized a priori (see, especially, p. 415). I delay detailed discussion of this point until §3.3, where I return to Cassirer's account of the a priori in depth. Ryckman (2005) and Heis (2012) both also ascribe a version of the relativized a priori to Cassirer.

understanding—with which constitutive principles are intimately related—are necessary is central to Kant's philosophy. This means that accounts of the relativized a priori have all sought to make more fundamental revisions to the notion of constitutivity that make it less dependent upon Kant's account of human experience in terms of human faculties. Reichenbach understood constitutive principles as being those that made possible a coordination between uninterpreted axiomatised mathematics and physical reality; Cassirer rejected the distinction between understanding and sensibility and emphasised the constitutive function of the regulative a priori as a condition of the possibility of having knowledge of objects at all.

Cassirer and Reichenbach represent two divergent methodologies for seeking to apply Kantian insights to contemporary philosophy. Reichenbach took the central Kantian claim to be that constitutive principles were *synthetic* a priori. So, in order to retain a role for such principles he argued that they should be relativized and understood as a priori only from the perspective of a particular theory. Throughout, following convention, I refer to this approach to Kantianism as the constitutive reading of Kant: it takes Kant's fundamental insight to be that our experience of the world is constituted by relativized synthetic a priori principles. This is to be contrasted with the regulative approach of Cassirer. The regulative approach emphasises the regulative role of reason in Kant's philosophy and seeks to develop an understanding of contemporary science that is based on regulative principles. This is the version of Kantianism that I will ultimately advocate.

Whichever approach is to be preferred, though, a satisfactory Kantian account of science must meet two challenges: it must provide an account of the rationality of scientific theory change and it muse also provide an interpretation of constitutivity. These two challenges will be important throughout, so in this introductory chapter, let us spend a little time spelling the challenges out.

B. Two challenges for a Kantian philosophy of science

In developing a Kantian philosophy of science that is fit for the modern era, we face two main challenges:

(CR) *The challenge of rationality*: How can it be rational to abandon an established conceptual framework in favour of a new one?

(CC) *The challenge of constitutivity*: Is it possible to make sense of Kant's notion of constitutivity in the context of modern physics?

CR deals with the problem of the rationality of theory change. This, of course, is not a problem that is unique to the Kantian, but it is a problem that is particularly sharp for that approach. This is for two main reasons. First, for Kant the objectivity of Newton's account of gravitation was grounded upon the relationship between Newton's laws of motion and the pure concepts of the understanding: that is, when we carry the concept of matter through the categories of relation we derive the laws of motion as a matter of necessity. So, for Kant, constitutive principles are necessary. When, for Kant, the objectivity of science is grounded upon the necessity of its constitutive principles, how is it possible to remain "Kantian" while allowing that constitutive principles can rationally change?

Second the Kantian approach involves commitment to the idea that humans only have epistemic access to the objects of experience and it is these that are described by a scientific theory. This means that common, realist, explanations of theory change are not available to the Kantian. For example, a Kantian response to this challenge cannot appeal to the idea that new theories are closer to the truth than old theories. Similarly it is difficult for a Kantian to appeal to structuralist explanations of continuity across theory change to ground the rationality of abandoning one framework for another.[5] Instead a Kantian account must provide an *internal* rationale for theory change.

Kant's understanding of constitutivity is intimately connected to his account of human experience in which he divided the intellect into the independent faculties of sensibility, understanding and reason. CC is concerned with whether it is possible to separate the idea of constitutivity from Kant's account of human experience. There are two broad types of approach to this endeavour that I consider. First, Friedman, following Reichenbach, seeks to reconceive Kant's distinction between sensibility and understanding

[5] The relationship between Kantian philosophy and structuralism is more complex than this. It is often suggested that Kantian philosophy, especially Cassirer's work, presages a form of structural realism. Ladyman (2009) suggests that epistemic structural realism can be traced back to Kant through Poincaré's Kantian influence. Ladyman's "Kantian epistemic structural realism" draws upon Langton's (1998) account of Kant's philosophy, which reads Kant as advocating humility as an epistemic virtue: I will argue that Kant's work contains richer insights than this. Gower (2000) also connects Kantian philosophy with structural realism, arguing that Cassirer should be read as anticipating the epistemic version of structural realism. It is, though, not just the epistemic version of structural realism that has been linked with Kantian philosophy: see (French, 2001), (French and Ladyman, 2003) and (Cei and French, 2009) for arguments to the effect that Cassirer's philosophy anticipates *ontic* structural realism. It is particularly clear in French's work—especially his (2001) and (Cei and French, 2009)—that there is some tension between a reading of Cassirer that seeks to attribute to him a version of structural realism and Cassirer's own explicit rejection of realism in favour of idealism. I discuss the relationship between Kantianism and ontic structural realism in §5. For my present purpose it suffices to say that any attempt to explain the success of theories on the grounds that they accurately capture the structure of the world and to use this as a basis to justify the rationality of theory change is not straightforwardly available to the Kantian.

and, in so doing, to relativize Kant's synthetic a priori. An alternative approach—characteristic of the reading of Kant that emphasises the regulative role of reason in his philosophy—is to abandon the attempt to draw a fundamental distinction between understanding and sensibility, and instead seek to historicise Kant's transcendental logic. On this approach the goal is to identify a general logical structure that is common to all scientific knowledge throughout its development: it is this that is judged, retrospectively, to have been constitutive of the entire sequence of scientific theories.[6]

If my account is to represent a satisfactory Kantian approach for contemporary philosophy of science, it must meet both CR and CC. In what remains of this chapter I develop these two challenges in more depth and explain why they are of such central importance.

C. The challenge of rationality

Since Kuhn's *The Structure of Scientific Revolutions* (1996 [1962]) there has been considerable emphasis in the philosophy of science on theory change: this is one of the most significant legacies of Kuhn's work. When Kuhn began working on *Structure,* history of science was a relatively young discipline and Kuhn perceived there to be a naive philosophical understanding of the development of scientific theories. It is not clear that there was a precise and well-defined philosophical account of the development of scientific theories to which *Structure* was intended to provide an alternative: instead, as Richardson (2007) argues, it seems that Kuhn primarily understood himself as responding to the view of science presented in the popular writings of the logical empiricists.[7] In particular, Kuhn understood the philosophy of his day to be committed to the idea that there was some form of neutral observation language against which theoretical statements were to be judged (1996, pp.125-6). The consequence of this is that if there is an observed fact that is explained by any given theory, then any successor theory that accounts for the same observational fact is commensurable with the earlier theory: that is, there is a theory neutral realm of observational statements that can adjudicate the rationality of scientific development. Richardson emphasises that this is not a view of logical empiricism that would have been

[6] This, at least, is how Friedman (2000) characterises regulative Kantianism. I argue in §3 that, while this account is correct in its broad detail, more can be said about the idea of constitutivity on this approach.

[7] This is indicated very clearly in an interview that Kuhn gave in 1995 (see, Richardson, 2007, p. 361), in which Kuhn stated "it was against that sort of everyday image of logical positivism—I didn't even think of it as logical empiricism for a while—it was that that I was reacting to when I saw my first examples of history". Richardson offers a few works as an example of the sort of popular image of logical positivism that Kuhn may have intended to respond to and pays particular attention to Frank's *Relativity: A Richer Truth* (1951) and Reichenbach's *Rise of Scientific Philosophy* (1973 [1951]).

unique to Kuhn, derived as it was from the many popular writings of leading logical empiricists.

Kuhn set about correcting this image of the development of science by providing a more careful historical account of the development of science than the logical empiricists were understood to have provided.[8] Kuhn, impressed by the work of Koyré among others, sought to provide a history that understood "what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today" (1996, p.viii). This line of thinking led Kuhn to draw a distinction between periods of normal science and periods of revolution in science. In a period of normal science the research community accepts a particular paradigm and conducts its research within this framework. It is not entirely clear how Kuhn understood the idea of a paradigm, however, I favour understanding the notion—within the context of *Structure*—in a methodological fashion. That is, I understand Kuhn's notion of a paradigm as being primarily defined by its set of exemplary puzzle-solutions.[9] Kuhn introduced this understanding of a paradigm as follows:

> Close historical investigation of a given specialty at a given time discloses a set of recurrent and quasi-standard illustrations of various theories in their conceptual, observational and instrumental applications. These are the community's paradigm, revealed in its textbooks, lectures, and laboratory exercises. By studying them and by practicing with them, the members of the corresponding community learn their trade. (1996, p.43).

The idea is that, during a period of normal science, new practitioners are trained in the problem-solving methods of the tradition by use of standard examples—whether these be problems in a text book or standard experiments to be carried out in a lab. This training embeds a particular set of methodological practices within a community. The development of a new theory requires the development of new methodological practices: the question of the rationality of theory change then becomes one of how it could be that a group of practitioners would abandon their old methodological practices and accept new ones.

Friedman interprets the threat of incommensurability in a somewhat different fashion. He emphasises Kuhn's later understanding of a paradigm, which is much closer to Carnap's idea of a linguistic framework.

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[8] The popular writings, especially of Frank and Reichenbach, were widely scorned for their lack of attention to historical detail. Cohen's view, expressed in his review of Reichenbach's (1973), is typical: "Since the armory of the "scientific philosopher" appears to include so many episodes from the history of science, I must admit to prejudice in favor of having the facts correct to begin with…It seems a pity that a work that contains so much about the history of science and that is devoted to replacing "error" by "truth" should itself attempt to find truth by repeating error." (Cohen, 1951, pp. 328-9).

[9] Kuhn emphasises this aspect of paradigms (1996, p. 187ff.) and Bird (2000, ch. 3) also emphasises this feature of the notion.

> Though it is a more articulated source of constitutive categories, my structured lexicon resembles Kant's a priori when the latter is taken in its second, relativized sense. Both are constitutive of *possible experience* of the world, but neither dictates what the experience must be. Rather, they are constitutive of the infinite range of possible experiences that might conceivably occur in the actual world to which they give access. (Kuhn, 1993, p.331)

Here Kuhn links his idea of structured lexicons—which was how he had come to understand his paradigms—to the relativized constitutive a priori. The idea is that constitutive principles are conditions for the possibility of the experience of the world, but they do not necessitate our own particular experience of the world.

What does it mean to change paradigms on the above understanding? Accepting a new paradigm, so understood, would involve accepting new conditions of our possible experience of the world. This would entail that some genuine possibilities according to the old paradigm are no longer possibilities on the new paradigm and, further, that the new paradigm could suggest that some things that were impossible on the old paradigm are now considered possible on the new one. The challenge of rationality—for the Kantian—is to explain how one can rationally accept the new set of possibilities from the perspective of the old paradigm: the new paradigm does not even represent a possibility.[10]

A proper account of science must explain the process by which an old theory is rejected and a new theory accepted. I take it that an account of science that treats this process as rational and concerned with scientific evidence should be preferred to an account that leaves the process either as irrational or as primarily driven by considerations that are not strictly to do with the theories (e.g. social considerations).

If we consider the challenge in terms of Kuhn's "structured lexicons" it is immediately clear why this will be difficult for a Kantian. Constitutive principles define the space of physical possibilities: this means that there is no possibility of our having an experience that directly contradicts our constitutive principles. It is conceivable that there will be experiences that do not fit with the current empirical system that is defined by the constitutive principles, but these can exist as anomalies and do not necessitate the adoption of new constitutive principles. The advance of Mercury's perihelion is an example of this sort of anomaly: it was a possible experience that did not fit with the empirical theory given

[10] This is precisely how Friedman understands CR: "Our problem, then, is to explain how transition from one scientific paradigm or constitutive framework to another can be communicatively rational, despite the fact that we are in this case faced with two different and even incommensurable "logical spaces". Moreover, our commitment to a relativised yet still constitutive conception of the a priori only makes the problem more difficult. For this commitment implies that there is an important sense in which we must agree with Kuhn that successor paradigms, in a genuine scientific revolution, are actually non-intertranslatable: the later paradigm, from the point of the earlier paradigm is not even a coherent possibility. How, therefore, can it ever be (communicatively) rational to accept the later constitutive framework?" (Friedman, 2001, pp. 95-6)

by the constitutive principles of classical mechanics, but on its own this was not enough to justify abandoning the old conceptual framework. If experience cannot contradict constitutive principles and empirical anomalies are insufficient to justify rejecting constitutive principles, what is left that can provide an account of the rationality of theory change?

D. The challenge of constitutivity

The idea that certain concepts and principles are constitutive of experience was a central part of Kant's critical project: constitutive concepts and principles are those that make objective knowledge possible.[11] In the *Critique of Pure Reason*, the pure concepts of the understanding—the categories—are constitutive of experience in general.[12] Kant's characteristic claim was that the objects of experience are not simply given to us: objects of experience are constructed from the manifold of intuition and the pure concepts of the understanding. The task of the pure concepts of the understanding is to order the manifold of intuition and make possible the cognition of unified objects. Without the pure concepts of the understanding, we would not be able to perceive any objects at all. It is in this sense that the pure concepts of the understanding are constitutive of experience: without the pure concepts of the understanding to give form to the manifold of intuition it would be impossible to have any knowledge of the objects of experience whatsoever.

Kant understood the pure concepts of the understanding to be *necessary*: their necessity being grounded in the very possibility of objects. However, they were not to be understood as being necessary in a merely logical, or analytic, sense; rather, the pure concepts of the understanding are necessary in a transcendental sense. The distinction between logical and transcendental necessity is built upon a distinction that Kant drew between general and transcendental logic. General logic is itself divided into dialectic and analytic logic, but let us just consider analytic logic so as to make clear the significance of the distinction between general and transcendental logic. Analytic logic is based upon the principle of non-contradiction; it does not allow any judgment regarding objects at all. Transcendental logic, on the other hand, "concerns itself solely with the laws of understanding and reason solely in so far as they relate a priori to objects" (A57/B82): the

[11] In this section I give a very brief summary of Kant's understanding of the idea of constitutivity and its role in scientific knowledge so as to clarify why it is both important and difficult for contemporary Kantian approaches to philosophy of science to provide an account of constitutivity. Detailed discussion of Kant's understanding of constitutive principles is given in §2.

[12] There are four categories—of quantity, quality, relation and modality—and each is sub-divided into three pure concepts of the understanding. The pure concepts of the understanding are concepts such as that of unity, of substance and of cause and effect. See (A80/B106) for the complete table of categories.

pure concepts of the understanding are necessary in a transcendental sense just because, without them there could be no cognition of objects. This is what is meant by the claim that Kant's constitutive pure concepts of the understanding are *synthetic a priori*.

Kant's philosophy of science was intimately related to the architectonic of knowledge that he had developed in the *Critique of Pure Reason*. He argued that constitutive principles were necessary in order to secure the objectivity of the laws of nature. In the *Metaphysical Foundations of Natural Science* (2004 [1786])—Kant's major critical-period work on the philosophy of science—he argued that Newton's laws of motion and the principles of Euclidean geometry were constitutive of the empirical law of gravitation.[13] Both the laws of motion and the principles of Euclidean geometry were understood as synthetic a priori knowledge. In the case of the laws of motion, this was because they could be derived by carrying the concept of matter as possessing "moving force" through the category of relation. In the case of the principles of Euclidean geometry this was because they could be known through construction in pure intuition. So, for Kant the possibility of objective knowledge depended upon the role of constitutive principles in the construction of experience. These principles were synthetic and a priori: they could be known independently of experience and they were transcendentally necessary. The transcendental necessity of the constitutive principles was crucial in explaining the objectivity of science.

It should be clear from this quick sketch of Kant's position that the idea of constitutivity—i.e. the idea that certain principles are known prior to experience and play a crucial role in securing the objectivity of knowledge—played a vital role in the Critical project. Any would-be Kantian philosophy of science must provide an account of constitutivity: i.e. it must provide an account of that which makes objective knowledge possible.

This task is far from straightforward. As we have seen, Kant's understanding of constitutivity is intimately connected to his understanding of the human intellect as being divided into the sensibility and the understanding. For Kant, this understanding of the human intellect was itself an answer to the main transcendental question as to how synthetic a priori judgments were possible. The most prominent synthetic a priori judgments, for Kant, were those of mathematics: e.g., Kant argued that 5+7=12 is a synthetic a priori judgment, as are geometrical judgments. Developments in both mathematics and physics, though, make it difficult to understand mathematical judgments as being synthetic a priori. Modern mathematics gives us a much richer notion of analyticity than that which was available to Kant, and this means that arithmetical judgments can now

[13] I examine the sense in which these principles were constitutive of the law of gravitation on Kant's account in detail in §2.

be understood as analytic rather than synthetic. Furthermore, the development of general relativity seems to have shown quite conclusively that the geometry of space is not to be stipulated prior to experience; on the contrary, it is a purely empirical question. If these judgments are no longer to be understood as synthetic a priori judgments, then this line of argument for Kant's account of the human intellect—and its associated account of constitutivity—would seem to be entirely undermined. How are we to provide an account of constitutivity that is suited to account for the objectivity of contemporary science, when this very science seems to undermine the motivation for explaining science in terms of constitutive principles in the first place?

I consider two ways in which this challenge might be addressed: the first is influenced by the work of the early logical positivists, the second is influenced by the Marburg School of neo-Kantianism. As we have mentioned, one of the primary problems with Kant's account of the synthetic a priori was the fact that by the start of the twentieth century Hilbert and Russell had developed a logic that seemed able to provide an analytic account of mathematical judgments. These accounts of logic came with a platonistic philosophy according to which logical objects were understood to be abstract entities that the mind came into contact with through passive reception. The effect of this was to create a sharp distinction between logic (and mathematics, since this could be derived by the new theory) and science: this significantly broadened the scope of that which could be considered analytic. Now, analytic judgments were understood to include logical and mathematical judgments; synthetic judgments were those which required experience.

This distinction between the realms of logic (and mathematics) and science was fully endorsed by the logical empiricists. It was now very difficult to see how a proposition can be both synthetic and a priori: after all, the synthetic is now just that which is given in experience. However, it was still possible to provide an account of constitutivity on this view. Kant's distinction between the understanding and sensibility had been replaced by a fundamental division between mathematics and experience. Scientific knowledge required that the two separate realms be coordinated: constitutive principles become those principles that coordinate mathematical formalism with physical experience.

This is how Friedman understands constitutive principles in his *Dynamics of Reason* (2001). However, as we shall see in §1.3, it is very difficult to secure the syntheticity of the a priori on this approach and Friedman is quite clear that he wishes to retain the syntheticity of the Kant's constitutive principles while relativizing them. So, for Friedman to answer CC he must provide an account of the syntheticity of constitutive principles. It is here, I suggest, that his account faces the most serious difficulties.

This type of objection is precisely the same as the objection that Schlick raised against Reichenbach's understanding of the relativized a priori. Schlick, as detailed in §1.3.1.1, understood scientific knowledge as a coordination between the physical realm and the purely formal realm of mathematics. Schlick accepted that constitutive principles were needed in order to successfully coordinate these two realms. However, he denied that constitutive principles should be viewed either as a priori or synthetic (1978b, p.333). Schlick does not accept that a priori can mean anything other than necessary and true for all time, and it was clear that constitutive principles had changed, so they clearly could not be a priori. Why did he deny the syntheticity of constitutive principles? Schlick understood Poincaré's work to have fatally undermined the Kantian idea of the faculty of pure intuition. This was because Kant took this faculty to necessarily have the structure of Euclidean geometry: Poincaré, though, had shown that there were three possible geometries that could describe the space of experience and that there was no possible experience that could enable us to choose between these three spaces.[14] Poincaré argued that we must select one of these geometries to treat as the geometry of experience on the grounds of mathematical simplicity alone. This is an analytic choice because the axioms of the chosen geometry are set out as definitions prior to experience. Poincaré understood the same process to be at work in the physical sciences as well: he argues that there are several versions of each of Newton's laws that are compatible with experience and the principles that are ultimately chosen are done set out as definitions and experience cannot give us definitive reason to pick one definition over another. So, Schlick, following Poincaré, understood constitutive principles to be analytic conventions in the sense that they are merely definitions that permit us to connect mathematics to experience. The problem posed by Schlick, then, is that while an account of constitutivity can be given in terms of constitutive principles, it does not amount to a *Kantian* account of constitutivity in any sense.

Friedman's first argument for the relativized a priori in his *Dynamics of Reason* is susceptible to precisely this charge, because he accepts the same understanding of mathematics as advocated by Schlick. Since his (2001), Friedman has attempted to distance himself from this understanding of mathematics and develop a historicised account of constitutive principles that makes it possible to defend a sense in which they are synthetic. In my discussion of Friedman's account of constitutivity I argue that he does not provide a satisfactory answer to CC because he is unable to secure the syntheticity of the relativized a priori..

[14] See §3.1 for a detailed explanation of this feature of Poincaré's philosophy.

E. Constitutive or regulative principles?

The central question with which I am concerned throughout, then, is as to whether a contemporary Kantian philosophy of science can provide more convincing answers to CR and CC by emphasising constitutive or regulative principles. I begin by outlining Friedman's attempt to answer CR and CC within the framework the relativized synthetic a priori. Friedman seeks to answer CR via a particular historical take on the development of general relativity which affords philosophy a central role as a meta-paradigm that mediates the rationality of the adoption of new theories. Friedman seeks to answer CC by arguing that this historical narrative can be given a fruitful philosophical interpretation if we assign a constitutive role to relativized synthetic a priori principles. I argue that Friedman's argument fails because he does not provide a satisfactory sense in which his constitutive principles are synthetic.

I do not think that we can profitably understand contemporary physics in terms of the synthetic a priori. As such, I think it is preferable to pursue the approach to neo-Kantianism advocated by Cassirer and the Marburg School: i.e. historicising Kant's transcendental logic. This approach emphasises Kant's account of the regulative role of reason—especially in providing the regulative ideal of unity—and downplays the role of constitutive principles. As Friedman points out (2000, p.117), this means that it is somewhat unclear that a regulative Kantianism can provide answers to CC: as we have seen, this claim was characteristic of Kant's approach and it is difficult to see how a philosophical position can be meaningfully Kantian without a notion of constitutive principles. Furthermore, without relativized constitutive principles, Friedman argues that it is impossible to provide an account of the prospective rationality of a new theory. If we are to defend a meaningfully Kantian philosophy of science on the regulative approach, then, one of the main challenges will be to show that a regulative Kantianism is compatible with a notion of constitutivity.

In §2 I take the first steps towards providing an account of constitutive principles that is compatible with a regulative Kantianism by examining the origins of constitutive and regulative principles in Kant's account of Newtonian physics. In particular I seek to emphasise the crucial role that both constitutive and regulative principles have to play in Kant's derivation of the law of gravitation. This is important for the dialectic of my overarching argument because it shows that, for Kant, the synthetic a priori was no more important to his architectonic than the purely regulative role of reason. As such, in light of the development of physical theories that are incompatible with Kant's philosophy, emphasising either constitutive or regulative principles are equally valid methods to apply key Kantian insights to contemporary science.

I then turn my attention to Cassirer's neo-Kantian structuralism, which—I argue—can form the basis of a satisfactory contemporary account of philosophy of science. In §3 I am chiefly concerned with arguing—*contra* Friedman—that Cassirer's philosophy of science can provide satisfactory answers to CR and CC. Cassirer's answer to CC has two parts: (i) he seeks to secure a role for relativized a priori principles, although these are not understood as synthetic a priori principles in the sense required by Friedman and (ii) he argues that objects of experience are constituted by the laws of a theory. I argue that Cassirer's answer to CR is best understood as claiming that it is rational to adopt a new theory if it has a broader invariance group than the old theory. This, I suggest, can form the beginning of an account of theory change, but more needs to be said to develop a satisfactory regulative answer to CR.

This is the task of §4, which takes the form of a case study of the development of general relativity. In this chapter I develop a regulative answer to CR that serves as an alternative to the answer proposed by Friedman. I suggest in §5 that it will also help in providing a regulative answer to CC: in this section I focus on the idea of law-constitutivity and argue that—coupled with Friedman's idea that physical principles play a historical role in making laws possible—a Kantian account of objects provides a viable alternative to contemporary structuralist accounts of objects.

In so doing, I suggest, it is possible to do justice to those aspects of Friedman's account which do seem to capture certain features of the development of science—especially the emphasis on the role of constitutive principles—within the framework of a regulative Kantianism.

1

Friedman on the relativized a priori and the rationality of science

1.1. Introduction: Friedman and the relativized synthetic a priori

By far the greatest single contributor to the attempt to reintegrate key Kantian insights into contemporary philosophy is Michael Friedman. In this chapter I introduce his account of the relativized yet still constitutive a priori. Friedman's account of the nature of scientific theories is a development of his earlier work on the philosophy of the early logical empiricists. Friedman's reconstructive account of the early works of Moritz Schlick, Hans Reichenbach and, especially, Rudolf Carnap has proven hugely influential in reversing the perception—prevalent at the end of the twentieth century—of logical empiricism as a deeply flawed research programme. Friedman (1999) showed that the early work of each of these key logical empiricist figures was deeply influenced by Kantian considerations.

In his *Dynamics of Reason* (2001) Friedman sought to develop his reconstructive work on logical empiricism into an account of conceptual change in science. The project can be seen as addressing two central questions. The first is as to how theoretical concepts acquire empirical content. Friedman answers this along Reichenbachian lines by appealing to the relativised a priori: i.e. some empirical content must be assigned to theoretical concepts to ensure the concept's empirical applicability. In his (2001) Friedman identifies two philosophical developments since Reichenbach that pose the most significant obstacles to defending a version of the relativized a priori: Quinean epistemological holism and the Kuhnian account of scientific revolutions. Epistemological holism questioned any distinction between constitutive and empirical principles and Kuhn's historiography of science accepted the importance of conceptual frameworks but insisted that precursor and successor frameworks were radically incommensurable.

The *Dynamics of Reason* is best understood as an attempt to address these concerns. Against Quine Friedman argues that scientific theories are better understood as having a coordinative part to link abstract theoretical concepts with empirical content. Against Kuhn he suggests that there is a role for philosophy in securing commensurability between paradigms. Philosophy, he claims, acts as a "meta-paradigm" in which ideas from competing paradigms can be discussed and this allows scientists of differing paradigms to have meaningful discourse. Friedman, then, ultimately argues that science has the following, tripartite, structure.

1. The base level consists of empirical laws which are directly tested by rigorous experimentation.
2. The intermediate level consists of the set of constitutive a priori principles that enable empirical testing.
3. A final level consisting of philosophical meta-paradigms which motivate and sustain scientific revolution.

Since the publication of the *Dynamics of Reason*, Friedman's notion of the relativized a priori has sparked substantial debate,[15] in response to which Friedman has modified his position (See his 2008; 2010a; 2012). Friedman still defends a version of the constitutive a priori, and still maintains that philosophy acts as a meta-paradigm that secures the rationality of scientific developments. The most notable change is that Friedman now seeks to distance his understanding of the constitutive a priori from Reichenbach's, whose philosophy Friedman now understands as relying on a mistaken view of the relationship between mathematics and physics. Instead Friedman now advocates a historicised account of the constitutive a priori.

In this chapter I clarify Friedman's answers to CR and CC and explain why the alternative approach I seek is to be preferred. Friedman's explanation of the rationality of science hinges upon a historical narrative according to which the philosophy of Helmholtz, Mach and Poincaré drove the development of relativity. I examine Friedman's historical argument and identify a number of concerns with it, which I return to discuss in §4. I then turn my attention to Friedman's account of the syntheticity of his constitutive principles. I trace the roots of this idea back to the early logical empiricists and explain why Reichenbach's account of constitutive principles cannot form the basis of a synthetic constitutive a priori. I then introduce Friedman's historicised version of the constitutive a priori. While this represents a more promising approach than that pursued in Friedman's (2001), some concerns remain: in particular I suggest that Friedman places too much emphasis on seeking to reinterpret the Kantian faculty of sensibility and too little emphasis on the regulative role of reason in science. In the following chapters I seek to develop a

[15] See (Domski and Dickson, 2010) and (Suarez, 2012) for substantial edited volumes discussing Friedman's work. See also (Ryckman, 2005), (DiSalle, 2002a and 2006) for other detailed discussion of Friedman's central ideas.

Kantian approach for contemporary philosophy of science that builds upon Friedman's highly innovative account while correcting these shortcomings of it.

1.2. Friedman's account of the rationality of science, or: Einstein's delicate dance

Friedman answers CR by means of a historical narrative: i.e., he argues that Einstein's development of general relativity is rational because of his engagement with the philosophies of Helmholtz and Poincaré.[16] The main challenge for the rationality of the development of general relativity, from Friedman's perspective, is to explain the process by which a four-dimensional, variably curved geometry became a "real"[17] or "genuinely physical"[18] possibility. The importance of philosophy is that it provides an arena of discourse that is informed by, yet distinct from, ordinary scientific practice. The meta-scientific reflection that is characteristic of philosophy of science serves to develop a stock of paradigm-independent concepts that are accessible to all scientific practitioners and which can be used to develop new scientific theories.

Friedman identifies two aspects of nineteenth century meta-scientific reflection on geometry as each playing a vital role in the development of the space-time of general relativity: Poincaré's conventionalist methodology—especially his emphasis on the elevation of scientific principles—and Helmholtz's empiricist understanding of physical geometry. First, Friedman argues that, in introducing both his definition of simultaneity and the equivalence principle, Einstein sought to elevate empirical facts to the status of principles. Second, Friedman emphasises the role that Helmholtz's account of geometry and the equivalence principle play in the rotating disk thought experiment in making four-dimensional space-time a genuine physical possibility. Friedman argues that the development of general relativity was only made possible through Einstein's engaging in this "delicate dance" between Helmholtz and Poincaré (2010a, p.710). In this section I seek to clarify what Friedman means by this, and raise some initial concerns about the accuracy

[16] The most detailed version of Friedman's historical narrative is found in his (2010a), which is an expansion of the main line of argument of his (2001). The main additions to his (2010a) narrative are greater detail in his account of Kant's philosophy of science, a longer discussion of the role of *Naturphilosophie* within his narrative and more emphasis on the influence of Ernst Mach's work in developing the idea of inertial frames. My main focus in this section will be on Friedman's account of the role of Poincaré and Helmholtz in providing Einstein with the philosophical tools to develop the theory of relativity. For further detail on this aspect of Friedman's narrative see his (2002).

[17] (Friedman, 2001, p.114)

[18] (Friedman, 2010a, p.725)

of his historical narrative. I detail this aspect of Friedman's answer to CR, before developing an alternative approach in the following chapters.

First, let us briefly summarise the two central philosophical ideas upon which Friedman's historical answer to CR rests. Helmholtz began his career as a physiologist and was particularly interested in the physiology of the senses. His work in this area led him to conclude that visual perception was entirely subjective in the sense that what is perceived is governed by the constitution of our retinas, rather than the nature of the external world.[19] For Helmholtz, this meant that we have no direct epistemic access to external reality and, of relevance here, the perceived spatial geometry need not correspond to physical geometry.

How was it possible, then, to say anything about the geometry of space? The solution lay in considering our ability to interact with the world: in particular our ability to construct an idea of three-dimensional space through our interactions with other bodies. This idea was made more precise by Felix Klein who understood geometry group-theoretically: i.e., the physically possible geometries are those which correspond with the group of rigid motions. This identified three geometries as physically possible—Euclidean, hyperbolic and elliptic—and the question became as to which one of these three is the correct physical geometry. Helmholtz resolved this problem by arguing that one could determine the correct description of geometry from among the three possible geometries by measuring, e.g., the interior angles of a triangle. Given his theory of perception, the matter was slightly more complex than this: the measurement procedure required the use of as many different senses as possible and a principle of trust in causal regularities of the world.[20] It is this claim—that the geometry of the world can be determined by measurement—that Friedman argues influences Einstein in coming to understand gravitation as curvature of four-dimensional space-time: I examine this claim in §1.2.2.

Poincaré is renowned for the claim that the fundamental principles of science—such as Newton's laws of motion—are "conventions and definitions in disguise" (Poincaré, 1905, p.138). Poincaré takes scientific knowledge to have a hierarchical structure; each level of the hierarchy makes possible those levels that are based upon it. The hierarchy begins with arithmetic, which he argues is a synthetic a priori science based on our capacity to represent an infinite iteration of a single operation. Next in the hierarchy is the concept of a continuous mathematical magnitude. This gives rise to the concept of space, which is

[19] This is because however he stimulated the retina—e.g. pressure, electricity—it was perceived as light.
[20] The idea is that in order to measure, e.g., length one would place unit measuring rods along a line and determine how many were used by both sight and touch. Both these senses are subjective on Helmholtz's account, so one cannot have absolute faith in their ability to accurately determine lengths, so Helmholtz also needed to trust that the world was such that regularities in our perception of the world corresponded to regularities in the world. See §1.3.2 for full discussion.

understood as a three dimensional continuous mathematical magnitude: the third level of the hierarchy is the study of the geometry of this space. The fourth level of the hierarchy is mechanics and this describes the motions of bodies in space.

Poincaré argued that within a level of the hierarchy it was necessary to make some, purely conventional, decisions: e.g., he claimed that the choice of Euclidean geometry and of each of Newton's laws was a matter of convention. Poincaré's reasoning can be seen most straightforwardly in the case of geometry. Like Helmholtz, Poincaré understood the construction of geometry to be based on our intuitive sensory experience of bodily displacement. Poincaré was also aware that this experience effectively underdetermines spatial geometry. However Poincaré gave a different answer to how we should determine which of the three possibilities represents the correct spatial geometry: he argued that we could not, in principle, ever answer this question. This is where the methodological practice of elevation first appeared in Poincaré's work. There are three geometries to choose from and Euclidean geometry is the most natural of these three in the sense that it allows for the simplest mathematical description of our experience. As such, Poincaré argued that it is elevated it to the status of a "convention or a definition in disguise" and is then held fixed as the geometry upon which the next stages of the hierarchy will be based.

This account of the conventionality of geometry is typical of Poincaré's treatment of all conventions: in the case of each convention we can identify the following three features and I suggest that these may serve as a set of necessary conditions for a principle to be conventional.

i. It is a natural idealisation of our sensory experience.
ii. There are other possibilities consistent with experience. And,
iii. Once accepted it can no longer be empirically tested and is, in that sense, irrefutable by experience.

Friedman argues that Einstein understood the equivalence principle and simultaneity as conventions in precisely this sense. It is important to stress (iii) because it is this that distinguishes Poincaré's process of elevation from simple empirical generalisation. In §1.2.1, I argue that, while Poincaré may have influenced Einstein's account of the conventionality of simultaneity, there is insufficient evidence to support the claim that Einstein understood the equivalence principle in this way.

1.2.1. Einstein's Poincaréan methodology: elevation in the cases of the light postulate and the equivalence principle

Friedman characterises Einstein—quite correctly—as something of a philosophical opportunist:[21] Einstein would pick those parts of realist, positivist and idealist philosophies that he thought may be useful and simply paid no attention to any unwelcome consequences of these philosophies. On Friedman's account, this philosophical opportunism afforded Einstein access to a set of ideas that he could use in order to develop a new conceptual framework within which to understand gravitation.

On Friedman's account it is Poincaré's methodology of "elevation" that is vital for Einstein: Friedman argues that both the light postulate and then the equivalence principle were developed through adopting and modifying Poincaré's methodology: Friedman characterises the development of both principles as arising through the elevation of empirical laws to the status of principles "to which our mind attributes an absolute value" (Poincaré, 1905, p.125). For the purpose of discussing Friedman's account of the development of relativity it is important to stress that principles that have been "elevated" should not be falsifiable by experience.

So, in the case of the light postulate, Friedman argues that the experimental law that is elevated to a principle is derived from the failure to detect the Earth's motion with respect to the aether; in the case of the equivalence principle the empirical law is the observed universality of free fall. In this section I examine Friedman's argument to the effect that the process of elevation played a vital role in Einstein's methodology. I have some doubts about Friedman's account: I certainly do not think that Friedman's conclusion is forced on us by the evidence he presents. I sketch an alternative understanding of Einstein's methodology that emphasises the regulative role of the ideas of invariance and theoretic unity, which I suggest enjoys better historical support.[22]

Let us begin by examining Friedman's argument that Einstein is implementing Poincaré's methodology. Einstein had read and discussed Poincaré's *Science and Hypothesis* between 1902 and 1904 as part of the curriculum of his Olympia Academy in Bern, so he would clearly have been well acquainted with Poincaré's philosophy while preparing *On the*

[21] It is important to point out this aspect of Friedman's account: while Friedman argues that philosophy plays a role as a meta-paradigm that provides an arena of paradigm-neutral debate about physical ideas that can help scientists overcome conceptual problems, he does not require scientists to have any deep commitment to the ideas that they take from the philosophical debate. In fact, quite the reverse is true: once the philosophical idea has served its purpose then it is discarded and need not become entwined with the theory itself. Friedman repeatedly stresses that philosophy is not a branch of science, and the scientist's ability to opportunistically adopt and discard philosophical ideas is part of the reason that the distinction between philosophy and science can be maintained. See (Friedman, 2010a, p.768 n. 208 and p.769 n. 211).

[22] See §4.

Electrodynamics of Moving Bodies (1905).[23] In his (2010a) Friedman begins his discussion of Einstein's work by quoting from (Einstein, 1905), and arguing that Einstein appealed to conventionalist methodology. The passage in question reads as follows:

> Examples of this sort, together with the unsuccessful attempts to discover any motion of the earth relative to the "light medium," suggest that the phenomena of electrodynamics as well as mechanics possess no properties corresponding to the idea of absolute rest. They suggest rather that, as has already been shown to the first order of small quantities, the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics are valid. We will elevate [*erheben*] this conjecture (whose content will be called the "principle of relativity" in what follows) to the status of a postulate [*Voraussetzung*], and also introduce another postulate, which is only apparently irreconcilable with it, namely, that light is always propagated in empty space with a definite velocity *c* which is independent of the state of motion of the emitting body. (Friedman's translation of Einstein, 2010, p.653. Cf. Einstein, 1905, CPAE2, pp.140-1).

Friedman interprets Einstein's stated intention to elevate the conjecture that the laws of electrodynamics and optics will be valid for all frames of reference to the status of a postulate as a clear sign that he is implementing Poincaré's methodology.[24] Friedman suggests that the same is true for the postulate that "light is always propagated in empty space with a definite velocity *c*". Although at the time Einstein had not been aware of the Michelson-Morley experiment to determine the motion of the Earth with respect to the aether, he *was* aware of the earlier experiments that had failed to detect any motion up to first order in v/c and claimed that he expected the first-order results to be confirmed for all other orders.[25] Some work needs to be done to clarify that Einstein had Poincaré's work in mind here: as it stands this passage is perfectly compatible with an empiricist reading of the development of these principles.[26]

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[23] The Olympia Academy was formed by Einstein, Maurice Solovine and Conrad Habicht in 1902, while Einstein was working at the patent office. It was an informal discussion group consisting of young physicists with an interest in philosophy. We know, from Solovine that the reading list contained—among others—Dedekind's *What Are and What Should Be the Numbers?*, Hume's *Treatise*, Mach's *Analysis of Sensations*, Mill's *System of Logic*, Pearson's *The Grammar of Science* and Poincaré's *Science and Hypothesis*. Einstein clearly had a great interest in philosophy, it is certainly plausible to suggest that the discussion of topics at the Olympia Academy may impact upon Einstein's methodology as a physicist. See (Miller, 1980, p.129 and p.139n) and (Howard, 2005, p.36) and references therein for details of the Olympia Academy.

[24] "It appears from Einstein's language, then, that he is here following Poincaré's methodology quite precisely" (Friedman, 2010a, p.654).

[25] This information is found in a footnote that Einstein added in 1913 for Lorentz's (1952) collection of essays on relativity.

[26] See (Norton, 2010) for a detailed empiricist account of how Einstein developed these principles. Norton's account has an interesting relationship to Friedman's: like Friedman, Norton argues that philosophy played a role in the development of special relativity, but he suggests that it is an empiricist philosophy—derived from Hume and Mach—that was most influential. This, then, does not support a challenge to the general answer to

Friedman is on firmer ground in his discussion of Einstein's definition of simultaneity. Einstein noted that there is seemingly an incompatibility between the principle of relativity and the light postulate: this is resolved by an analysis of the concept of simultaneity. In brief, Einstein's argument ran as follows. He considered two distant clocks and asked how we should synchronise them. His idea was that an observer at the first clock should send out a signal to the second clock, this could then be reflected and received back at the first clock. What time should we say that the first clock read when the light signal arrived at the second clock? Einstein's answer was that the natural way to define simultaneity is to say that the light arrived at the second clock after half of the time for the total journey (as measured on the first clock) had elapsed.[27] This understanding of simultaneity is not forced on us: we could, for example, assume that space is not isotropic and that there is a level of resistance to motion in a given direction.

Before looking at Friedman's understanding of the equivalence principle let us pause briefly to emphasise that (at least Friedman's understanding of) simultaneity meets the criteria for a convention set out above. First, that the speed of light is the same in all directions and independent of the speed of the source is a natural idealisation of the Michelson-Morley experiment. Second, there is—until gravitation enters the picture—a physically possible alternative that is also consistent with experience in Lorentz-Fitzgerald contraction. Third, in using the concept of simultaneity to *define* a new framework for space, time and motion Friedman takes it that Einstein puts the principle beyond empirical refutation.

Friedman's argument that Einstein understood the equivalence principle in the same fashion is as follows:

> In using the principle of equivalence to define a new inertial-kinematical structure, therefore, Einstein has "elevated" this merely empirical fact (recently verified to a quite high degree of approximation by Lorand von Eötvös) to the status of a "convention or definition in disguise"—just as he had earlier undertaken a parallel "elevation" in the case of the new concept of simultaneity introduced by the special theory. (Friedman 2010, p.709)

The claim that Einstein used conventionalist methodology in elevating the equivalence principle has an initial appeal: it would seem eminently rational for Einstein to repeat the same process that had led to success just a few years earlier. However Friedman does not

CR that Friedman provides—according to which philosophy plays a role to drive theory change—but it does raise problems for Friedman's argument that this history is chiefly derived from Kant.

[27] See DiSalle (2006, pp.103-11) for a more detailed account along these lines.

offer anything more than this claim that Einstein was simply repeating a method that had previously brought success.

The problem with understanding Einstein as self-consciously implementing Poincaré's conventionalist methodology in the case of the equivalence principle is that it is not clear that he understood the principle to be unfalsifiable. In the case of the definition of simultaneity employed in special relativity, there is a much clearer sense in which this is unfalsifiable: Einstein introduced simultaneity as a definition—simultaneity is not an objective feature of reality—and it is very hard to conceive of an experiment that would render this definition invalid. The equivalence principle is a very different type of principle. The equivalence principle does tell us something of the nature of objective reality—that the gravitational mass of a body is the very same property of a body as its inertial mass—and it is a straightforward matter to conceive of the experiment that would falsify the claim: all that is needed is to observe two objects falling at different rates in a gravitational field. While Einstein was convinced that the equivalence principle was an important physical principle, there is no reason to suggest that he understood it as unfalsifiable: if he thought that the principle could be falsified it is difficult to see how we can claim that he was deliberately implementing conventionalist methodology.

Howard (2010) argues that Einstein understood the equivalence principle as an empirical generalisation: [28] as such, it would be perfectly possible for the principle to be proven false. This claim is based upon Einstein's treatment of general relativity as a principle theory of precisely the same type as thermodynamics, in his well-known letter of 1919 to the *Times*. Einstein distinguishes between two types of theory: constructive and principle. Constructive theories are those that "attempt to build a picture of complex phenomena out of some relatively simple proposition" (1919, p.13), as the kinetic theory of gases does. By contrast, principle theories analytically derive conclusions from certain general principles.[29] So, for Einstein, a principle theory is one that begins with a frequently observed empirical regularity and then uses this to derive additional consequences. Howard takes this to imply that the claim that the equivalence principle has precisely the same status as the claim that there are no perpetual motion machines.

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[28] This impression is reinforced by the argument of (Norton, 2010). Here Norton examines Einstein's claim that the philosophy of Hume and Mach helped him in developing special relativity: he argues that all that Einstein meant by this is that these philosophers convinced Einstein that scientific concepts must be grounded in experience.

[29] "Their starting point and foundation are not hypothetical constituents, but empirically observed general properties of phenomena, principles from which mathematical formula are deduced of such a kind that they apply to every case which presents itself. Thermodynamics, for instance, starting from the fact that perpetual motion never occurs in ordinary experience, attempts to deduce from this, by analytic processes, a theory which will apply in every case." (Einstein, 1919, p.13.)

This would seem to imply that Einstein understood the equivalence principle to have the same status as the second law of thermodynamics. It would seem quite clear that the discovery of a perpetual motion machine would invalidate the second law of thermodynamics; surely it is equally clear that the equivalence principle would be falsified by the observation of one object falling in a gravitational field at a greater rate than another. The fact that these principles are stated as foundational does not mean that they cannot be proven wrong. For Howard (2010, p.349) Einstein placed so much confidence in the equivalence principle just because it was a generalisation from experience: its function is just to constrain the development of general relativity so that the theory does not violate this fundamental property of matter.

Friedman argues that, e.g., the Eötvös experiments do not amount to tests of the equivalence principle because all that such experiments do is test the claim that objects of different mass fall at the same rate in a gravitational field. The equivalence principle, as it features in general relativity goes beyond this claim and coordinates free-fall trajectories with space-time geodesics. This means that the Eötvös experiments "do not function as empirical tests of the equivalence principle" (Friedman, 2001, p.91). Friedman is, I think, right that the empirical observation that objects fall at the same rate in a gravitational field does not amount to the equivalence principle as it is employed in general relativity. However, I do not see how this prevents the Eötvös experiment testing the equivalence principle: if inertial and gravitational mass are not the same property of objects, then the equivalence principle—even understood as the claim that free-fall trajectories follow space-time geodesics—would seem to be straightforwardly refuted.

This, I take it, is good reason to doubt Friedman's claim that Einstein was self-consciously following Poincaréan methodology: conventions, in Poincaré's sense, are unfalsifiable and the equivalence principle would seem to be falsifiable. I return to discuss this aspect of Friedman's account in §4.2. I argue there that the equivalence principle is most profitably understood as resulting from an analysis of conceptual tension within Newton's account of gravitation. So, while I will ultimately disagree with Howard's reading, his empiricist reading of the development of the equivalence principle does seem preferable to Friedman's.

1.2.2. Variably curved space-time: Einstein between Helmholtz and Poincaré

The second time that, on Friedman's account, philosophical debate played an important role in shaping the conceptual development of relativity was in Einstein's coming to understand gravitation as curvature of four-dimensional space-time. Einstein engaged in a "delicate dance" between Helmholtz's and Poincaré's accounts of geometry and, in so doing, fundamentally reconfigured the relationship between geometry and space (Friedman, 2010a, p.710). Friedman's argument, in brief, is as follows. After Einstein had developed his theory of special relativity he sought to expand his theory so that it could account for gravitation. As we saw in the previous section, Einstein sought to achieve this via the equivalence principle. In its earliest formulation the equivalence principle is just the claim that uniformly accelerating frames of reference can be considered as inertial frames of reference equipped with a homogeneous gravitational field. This provided Einstein with a means to describe homogeneous gravitational fields using the physics of moving bodies. In 1912, Einstein published two papers that described the relativistic behaviour of bodies in this sort of (static) gravitational field. However, this was a very limited class of gravitational field and Einstein, naturally, sought to describe more complex, stationary, gravitational fields. The most simple example of such a field would be that which is generated by a uniform rotation: and this brought Einstein to consider the uniformly rotating disk. In order to make sense of this thought experiment, on Friedman's account, Einstein found himself drawing on elements of the philosophy of both Helmholtz and Poincaré.

Now, there is strong evidence that the rotating disk thought experiment was very important to the genesis of general relativity: in particular Stachel (1989; 2007a) has argued convincingly that the rotating disk thought experiment is the "missing link" in the development of general relativity. Why the need for a "missing link"? Prior to his *Entwurf* theory of gravitation (1913), Einstein had made no mention of the need for a non-flat space-time and he still had not made use of four-dimensional formalism in his theory.[30] However, by 1913 Einstein has fully integrated the use of the metric tensor to represent space-time curvature into his theory and there is no argument given in his *Entwurf* paper for the change of direction. The orthodox view now is, following Stachel, that the rotating disk thought experiment played a crucial role in the use of the metric tensor in this fashion.

Friedman draws our attention to Einstein's 'Geometry and Experience' (1954) as evidence for the crucial role of the rotating disk. This was a lecture that Einstein delivered to the Prussian Academy of Sciences in Berlin in 1922: in it, Einstein discussed the influence that his conceptions of mathematical and physical geometry had on the development of general relativity and he discussed how the philosophies of Helmholtz and Poincaré had impacted on his thinking. Friedman's interpretation of this lecture is central

[30] Einstein had, though, begun to familiarise himself with four-dimensional techniques in his 1912 exchange of papers with Abraham. See, e.g., (Einstein, 1912c) and (Renn, 2007b) for a discussion of the impact of Einstein's exchange with Abraham on the development of general relativity.

to his historical case: it is here that Einstein suggested that philosophical work on the foundations of geometry influenced his development of relativity. I will discuss this element of Friedman's account in the final chapter, for now I will detail Friedman's interpretation of this lecture and suggest some initial causes for concern with it.

Einstein began his lecture by drawing a distinction between mathematical and physical geometry. Mathematical geometry treats the fundamental entities of geometry—point, straight line, etc.—as intrinsically defined by the relevant geometrical axioms, which are free creations of the human mind and bear no relation to experience whatsoever.[31] On the other hand physical geometry requires us to coordinate the implicitly defined objects of mathematical geometry with physical objects. For example, Einstein suggests that we coordinate the <straight line> of axiomatised Euclidean geometry with the <practically rigid body>:

> It is clear that the system of concepts of axiomatic geometry alone cannot make any assertions as to the relations of real objects of this kind, which we will call practically-rigid bodies. To be able to make such assertions, geometry must be stripped of its merely logical-formal character by the coordination of real objects of experience with the empty conceptual framework of axiomatic geometry. To accomplish this, we need only add the proposition: Solid bodies are related, with respect to their possible dispositions, as are bodies in Euclidean geometry of three dimensions. Then the propositions of Euclid contain affirmations as to the relations of practically-rigid bodies. (1922, pp.31-2)

So, one of the chief aims of geometry is describe the spatial relationships between bodies. This is impossible using only mathematical geometry because the objects of which it treats do not exist in the physical world. So, in order to apply mathematical geometry to experience, we must coordinate the implicitly defined mathematical objects with physical objects. Einstein sought to do precisely this in coordinating <straight line> to <practically rigid body>. Friedman claims that Einstein understood physical geometry to be associated with Helmholtz's account: though Einstein does not explicitly mention Helmholtz in relation to this understanding it does seem plausible enough that this is the case.[32]

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[31] It is worth noting that Einstein explicitly associates this view with Moritz Schlick's account of mathematical knowledge "The matter of which geometry treats is first defined by the axioms. Schlick in his book on epistemology has therefore characterised axioms very aptly as "implicit definitions" (Einstein, 1922, p.30).

[32] The best evidence to support Freidman's claim is that Einstein in his (1925), in the context of a similar discussion, does mention Helmholtz in relation to this view: "Either one accepts that the 'body' of geometry is realized in principle by the solid bodies of nature, if only certain prescriptions are maintained regarding temperature, mechanical stress, and so on; this is the standpoint of the practicing physicist. Then a natural object corresponds to the 'interval' of geometry, and all propositions of geometry thereby attain the character of assertions about real bodies. This standpoint was represented especially clearly by Helmholtz, and one can add that without it establishing the theory of relativity would have been practically impossible. Or, one denies in principle the existence of objects that correspond to the fundamental concepts of geometry. Then geometry alone contains no assertions about objects of reality, but only geometry together with physics. This standpoint, which may be more perfect for the systematic presentation of a completed physics, was represented especially clearly by Poincaré. On this standpoint the total content of geometry is conventional;

Einstein claimed that this understanding of physical geometry was essential in his formulation of general relativity:

> I attach special importance to the view of geometry which I have just set forth, because without it I should have been unable to formulate the theory of relativity. Without it the following reflection would have been impossible : -In a system of reference rotating relatively to an inert system, the laws of disposition of rigid bodies do not correspond to the rules of Euclidean geometry on account of the Lorentz contraction; thus if we admit non-inert systems we must abandon Euclidean geometry. The decisive step in the transition to general co-variant equations would certainly not have been taken if the above interpretation had not served as a stepping-stone. (1922, p.33)

So Einstein's conception of physical geometry played an important role in the development of general relativity because it enabled him to understand that the geometry of a rotating system could not be Euclidean.[33] The crucial conceptual insight in relativity is that gravitation should be represented by curvature of four-dimensional space-time. Einstein claimed that this insight was prompted by considering the application of physical geometry to the rotating disk. To see why, begin by considering a stationary disk: we determine the ratio of the circumference to the diameter by counting how many unit measuring rods we place to cover the diameter and the circumference. If this disk is set into relativistic rotation the measuring rods placed along the diameter should not contract—because they are orthogonal to the direction of motion of the disk—whereas the rods placed circumferentially will contract because they are placed in the direction of motion of the disk. This means that we would need to place more unit measuring rods on the disk in order to cover the circumference, meaning that we would measure the ratio of circumference to diameter as greater than π: i.e., we would measure non-Euclidean geometry.[34]

The importance of Helmholtz in Friedman's narrative is clear: Helmholtz's account of physical geometry was precisely the understanding of geometry that Einstein needed in order to develop relativity. Where, though, does Poincaré enter the narrative? Einstein went on to acknowledge that this reliance on rigid rods, *sub specie aeterni*, could not actually be correct: so Einstein, ultimately agreed with Poincaré, but argued that for practical purposes—under certain conditions—one could treat bodies as if they were rigid and use them to empirically determine spatial geometry.

which geometry is to be preferred depends on how 'simple' a physics can, by its use, be established in agreement with experience." (Einstein, 1925. pp.18-9; translation from Friedman, 2010a, p.768 n. 209).

[33] I discuss the role of the rotation in the development of general relativity in §4.3.

[34] This is how Einstein understood the rotating disk thought experiment, however the situation is not quite this straightforward: in particular, why is it not the case that the rotating disk would contract as well, with the result that the same number of measuring rods would be required to measure a rotating disk as a stationary disk?

While the account of the development of general relativity that Einstein gives in his (1922) does lend weight to Friedman's historical narrative, we ought to be careful about attaching too much weight to this sort of retrospective rationalisation of the development of a theory. In this case there are especially good reasons for caution. Einstein's account of the distinction between mathematical and physical geometry closely follows the understanding of mathematical and physical knowledge that had been developed by the early logical positivists at the time and as had been championed by Schlick.[35] This is a concern because in the period 1916-1922 Einstein had spent some time engaging with philosophers who sought to make sense of what was meant by general relativity: he read and discussed Kantian interpretations of his theory and, while he initially welcomed such interpretations, by 1921 he had rejected these in favour of Schlick's realism.[36] This is cause to pause before taking Einstein's position in 'Geometry and Experience' to reflect his position in 1912: he essentially claims that the philosophical position that he had settled on in 1921—i.e. the distinction between mathematical and physical geometry as it had been drawn by Schlick—provided a crucial insight during the development of his theory back in 1912. This is not to say that Einstein's account is not accurate, but it does provide reason to be cautious about accepting Einstein's (1922) account as decisive.

As I have stressed, Friedman's historical narrative *is* his argument for the role of philosophy in the development of science: before we can accept his conclusions it behoves us to undertake a detailed study of Einstein's writings around the period in question—1912—to see if these corroborate the later account. I undertake such a study in §4.3. I argue there that there is no need to appeal to the geometry of Helmholtz and Poincaré in order to understand Einstein's solution to the problem of rotation. Furthermore, I argue that in 'Geometry and Experience', Einstein is more concerned with refuting Weyl's approach to geometry than Poincaré's. These two factors, I suggest, mean that we should not read Einstein's development of general relativity as being motivated by philosophical engagement with Helmholtz.

1.2.3. Philosophy and the rationality of science

Friedman's historical narrative is intended to provide sufficient evidence that philosophy has, throughout the development of space-time physics, played a crucial role in the

[35] I discuss this account of the distinction between mathematical and physical knowledge in connection with the work of Schlick and Reichenbach in §1.4.1.1. For the purpose of my point here it suffices to note that Einstein's understanding of geometry has clear parallels with Schlick's view.
[36] For an account of Einstein's attitude to the various philosophical accounts of relativity see (Howard, 2010). For an account of Schlick's Duhem-inspired realism see (Friedman, 1999, ch.1).

reconfiguration of the conceptual structure of space-time physics. The historical narrative is impressive in scope, beginning with Newton's theory, and taking us through Leibniz, Kant, *Naturphilosophie*, Helmholtz, Mach and Poincaré: the philosophical debates that accompany the development of theories of space (and time), for Friedman, provide a set of common questions and concerns that feed back into the physics and that ultimately mediate the rationality of theory-change. I have focussed in this section on just a small, though important, part of this narrative—the role that Friedman assigns the philosophies of Helmholtz and Poincaré in the process—and have suggested that Friedman's historical reconstruction may not be the most plausible available.

How, though, should we characterise the role of philosophy in the above narrative? Friedman argues that when we move from one scientific framework to another there is an intermediate stage in which aspects of the old framework are still being transformed while the new framework is not yet fully articulated. So, in a case of conceptual change there is necessarily a period of time during which there is no stable conceptual framework for scientists to appeal to: scientists in this revolutionary period are "caught in a deeply problematic (but nevertheless intensely fruitful) state of inter-paradigmatic conceptual limbo" (Friedman, 2001, p.115). The idea is that in this period philosophical ideas can be appealed to in order to try and work towards a new stable conceptual framework.

This, argues Friedman, is precisely how we should understand the influence of Helmholtz and Poincaré on Einstein. Poincaré's methodology of elevation enabled Einstein to develop the equivalence principle. In the early stages of the development of general relativity, the equivalence principle was applied to three-dimensional, flat space-times. From our current perspective on relativity, this is not how we would understand the application of the principle, but, at the time—before the new conceptual framework was in place—this was the appropriate way to apply the principle. The role of the rotating disk thought experiment, similarly, occurs at an intermediate stage. The philosophical debate between Helmholtz and Poincaré, as we have seen, centred around which of the three space-times of constant (or zero) curvature describe spatial geometry: Einstein relied on this debate at a crucial stage of the development of general relativity, but given that the final version of the theory describes gravitation in terms of a four-dimensional, variable geometry, there is an important sense in which the philosophical debate is rendered irrelevant by the new theory.

This is to be expected on Friedman's account. Philosophy is, for Friedman, emphatically not a part of science: it is a separate discipline whose debates are carried out in parallel to science. What is important is that it provides a series of settled questions that can be discussed in a theory-neutral way. For example, Helmholtz and Poincaré disagreed about *why* the space of Newtonian physics was Euclidean: is it because it was measured as Euclidean or because it was the most mathematically simple geometry? Newtonian physics provided no answer to this. However the philosophical debate could be drawn upon by Einstein while he was developing a new theory. Because the philosophical debate existed independently of science and because its application depended upon ideas available in the old conceptual framework the procedure by which Einstein transformed the old conceptual framework to create a new one is rational from the perspective of the old theory. This, in the end, is Friedman's answer to CR:

> **Friedman's answer to CR**: Philosophical debate is theory neutral, it is appealed to in periods of conceptual limbo in order to rationally transform aspects of the old conceptual framework and to create a new one.

While I have expressed some concerns over the detail of Friedman's argument, his broader claim about the role of philosophy represents an innovative and intriguing answer to CR.[37] I also would suggest that Friedman's historical narrative captures something of importance in the development of the theory of relativity: Einstein certainly read and took on board philosophical ideas and also placed great emphasis on the definition of simultaneity and—more strikingly—the equivalence principle.[38]

Friedman intends to do more, though, than just argue for a role for philosophy in providing a meta-paradigm that makes the process of theory-change rational: he argues first that it is a particularly Kantian philosophy that, historically, has been most useful in this regard[39] and, second, that there is an element of (transcendental) necessity in the process.

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[37] McArthur (2008) objects that on Friedman's account it remains the case that rationality needs to be constructed retrospectively. This is true to an extent, but misses an important nuance of Friedman's account. For Friedman there are two stages to the process of developing a new theory: first, there is the intermediary stage, where philosophy has a role to play and then there is the stage where a new paradigm is in place and its relation to the former paradigm must be made rational. The latter stage is only retrospectively rational: however the former stage provides an account of how theory change can be prospectively rational: when there is no fixed conceptual structure available, philosophical discourse provides a stock of ideas that permit a community of scientists to explore the rationality of a variety of possible conceptual frameworks. This is rational form the perspective of the old framework: a Newtonian scientist can consider Einstein's special relativity to be a conceptual possibility because, for Friedman, there is a chain of reasoning from Newtonian ideas, via Poincaré, to the principles from which the empirical side of Einstein's theory is derived.

[38] The importance of the equivalence principle to Einstein in developing general relativity is also noted by Norton (1989; 2007). This is particularly remarkable because at the time that Einstein was developing general relativity, the vast majority of Einstein's peers were willing to abandon the universality of free fall: Einstein's insistence on the equivalence principle was something of a peculiar obsession. I explore the role of the equivalence principle in general relativity in §4.

[39] "The relevant intellectual situation, on my account, essentially includes specifically philosophical elements that directly derive, by a series of what I call minimal (and in this sense unique) generalizations and extensions, from Kant's original transcendental analysis of the fundamental spatio-temporal structure framing Newton's physical theory. It is not merely that we can now discern, in Einstein's theories, principles which are analogous, in important respects, to what Kant took to be the (absolutely) transcendentally necessary

This aspect of Friedman's account, though, is best assessed in terms of his answer to CC. It is to this aspect that I now turn my attention.

1.3. Friedman, the relativized a priori and the legacy of the early logical positivists

Friedman answers CR by providing a historical narrative according to which the philosophy of Helmholtz and Poincaré played a crucial role in the development of general relativity. It comes, though, with a "philosophical gloss": the relativized constitutive a priori. Friedman's philosophical goal is to describe these historical developments within the framework of a reconceived version of Kantian philosophy that treats constitutive principles as constitutive only with respect to a given theoretic framework.[40] So, Kant's constitutive principles are now seen as constitutive only with respect to Newtonian physics: i.e., they function "as necessary presuppositions for applying our (changing) conceptions of space, time, and motion to our sensible experience, but they are no longer eternally valid once and for all" (Friedman, 2010a, p.697). Friedman's account of the role of philosophy in the development of relativity is then meant to show how the principles that were constitutive for Kant can start to change: Newton's laws of motion become a limiting case and the geometry of space becomes a matter for empirical investigation. This is possible, for Friedman, through using empirical laws to reconfigure the way that our conceptions of space and time are applied to sensible experience. The empirical laws that were used in this fashion become constitutive principles—the light postulate and equivalence principle—for general relativity.[41]

It is at this stage that Friedman must seek to provide an answer to CC: how is it possible to make sense of Kant's notion of constitutivity in the context of modern physics? Friedman provides a different type of answer to this problem in his more recent work (2009; 2010a; 2012) that he had previously in the *Dynamics of Reason* (2001). In his (2001), Friedman's philosophical analysis of his historical narrative effectively endorsed

principles of Newtonian theory. Rather, the principles now claimed to be (relatively) transcendentally necessary in Einstein's theories are themselves derived, in the context of successive new developments in both mathematics and the empirical sciences, from Kant's conception. Kant's original insight into the structure of Euclidean geometry, Newtonian physics, and, more generally, the state of both mathematics and the empirical sciences in the late eighteenth century, was so deep, and so systematic, that changes successively forced in one or another part of the Kantian framework in one or another new intellectual context…then reverberated throughout this framework in extraordinarily productive ways." (Friedman, 2010a, p.727)

40 See (Friedman, 2010a, p.696).

41 Friedman is clear that both the light postulate and equivalence principle are constitutive of general relativity because both have a crucial role to play in the rotating disk thought experiment that led Einstein to the insight that gravitation could be represented by space-time curvature. The light postulate is implicit in the application of the special relativistic claim that rigid measuring rods contract when moving at relativistic velocities.

Reichenbach's understanding of the relativized a priori as *axioms of coordination*. Reichenbach's relativized a priori was part of a broader account of the relationship between mathematics and experience: Reichenbach had taken from Schlick the idea that knowledge was coordination between uninterpreted mathematical formalism and physical experience. The purpose of axioms of coordination was to enable the application of mathematics to experience. The problem with Reichenbach's approach, as Schlick pointed out, was that it is not clear that axioms of coordination can be understood as *synthetic* principles. Instead, they seem to be more naturally interpreted as *analytic* principles, which is clearly seen in Carnap's account of the relativized a priori. This, ultimately, is why Friedman has abandoned much of the philosophical framework of the *Dynamics of Reason* in his more recent work.

The relationship between Friedman's work and that of the logical positivists is illuminating in that it clarifies an important feature of Friedman's answer to CC. That is, he does not seek just to secure a role for constitutive principles in our contemporary understanding of scientific theories: Friedman seeks to answer CC by relativizing Kant's *synthetic* a priori. Friedman's recent work, therefore, adopts a quite different approach from that advocated in his (2001): he now defends a historicised account of constitutive principles. By this I mean that he argues that a given principle is constitutive of a theory insofar as it played a historical role in making the empirical laws of a theory physically possible.

This section is structured as follows. First, I outline the understanding of the relativized a priori in the work of Reichenbach and Carnap. The goal here is to clarify precisely why the philosophical framework within which Reichenbach understood the relativized a priori can only support *analytic* constitutive principles. Second, I detail Friedman's account of the relativized a priori in his (2001) and show that it faces precisely the same problem as Reichenbach's axioms of coordination. Then I turn my attention to Friedman's current, historicised, approach to answering CC. In part I think that the current account is very helpful, but, I argue, it is not at all clear that it provides a means to salvage something of the synthetic a priori. This being so, I suggest that we should seek to answer CC without attempting to retain the synthetic a priori: the most promising alternative is the regulative reading of Kant according to which we should seek to salvage key Kantian intuitions by historicising his account of transcendental logic.

1.3.1. Reconfiguring the relativized a priori: a problematic picture

In introducing CC, I mentioned that Schlick had objected to Reichenbach's (1965 [1920]) formulation of the relativized a priori by rejecting the idea that there was any sense in which the principles could be considered a priori. Schlick did not take this to be an objection of any great substance, he characterised the dispute as terminological: he understood Reichenbach's relativized a priori just to be Poincaréan conventions mis-identified.[42] Reichenbach, too, took Schlick's objection to be one merely of terminology and eventually comes to refer to his relative a priori principles as conventions. Friedman's early efforts (1999; 2001) to secure a sense of syntheticity for the relativized a priori proceed by arguing that there was more at stake in this dispute than terminology: in accepting Schlick's terminology, Reichenbach also found himself accepting Duhemian holism. Friedman's early answer to CC is just to advocate a return to—and reconfiguration of—Reichenbach's work of 1920, before he (mistakenly) accepted Schlick's recommendation to treat his notion of the relative a priori as conventions.

The notion of the relativized a priori that Friedman defends in his (2001), then, is derived chiefly from Reichenbach. However Carnap's influence on Friedman's version of the relativized a priori is important too. This is because Friedman understands Carnap's L-rules as an explication of Reichenbach's version of the relativized a priori. However Carnap understands his L-rules to function analytically: Friedman, then, must explain what distinguishes his version of Reichenbach's a priori from Carnap's. I show how Friedman attempts to do this by tying the fate of Carnap's logico-linguistic frameworks to the fate of naturalism—construed just as the attempt to treat philosophy as a branch of science—and argue, in §4.1.3, that Friedman's (2001) version of the relative a priori ultimately cannot be distinguished from Carnap's.

[42] Discussing Reichenbach's work, Schlick wrote: "He reaches the conclusion that Einstein's theory is incompatible with the original doctrine of Kant, and proposes a transformation of the concept of the *a priori*, such that relativity theory will no longer contradict it, and the most important thesis of the Kantian philosophy will remain, as he thinks, intact. This thesis he professes to find in the insight that all knowledge becomes possible only through the logical presupposition of certain principles, which first constitute its object as such. Such principles he calls *a priori*, but dispenses with the mark of apodeicticity; hence they are not necessary, and the progress of knowledge can provide motives for modifying them. "*A priori* means 'priori to knowledge', but not 'for all time', and not 'independently of experience'" [Reichenbach, 1965, p.105]. In view of my earlier remarks…this strikes me as a total departure from the basis of the critical philosophy, and I should designate Reichenbach's *a priori* principles as conventions, in Poincaré's sense. Thus I cannot commend the author's terminology, but in substance I agree entirely with him on most of the essential points." (Schlick, 1978b, p.333).

1.3.1.1. The constitutive a priori in the work of Schlick, Reichenbach and Carnap

Friedman's account of the relativized a priori—certainly as it appears in his (2001)—can be profitably understood as an attempt to salvage Reichenbach's notion of the relativized a priori from Schlick's and Carnap's attempts to treat the notion in an analytic fashion. In this section I examine the emergence of the relativized a priori in Reichenbach's *Theory of Relativity and A Priori Knowledge* and explain why, ultimately, Reichenbach's work lacks the resources to serve as a foundation for would-be Kantian attempts to answer CC.

Reichenbach's account of coordination was deeply influenced by Schlick's interpretation of the nature of truth in logic and mathematics. Schlick (1978a [1910], p.83) argued that a proposition should be called true only when "it will always and under all circumstances be verified"—he called this the *principle of universal validity*. This principle applied first and foremost to mathematical and physical judgments: Schlick gives Euclidean geometry as an example of this type of a priori knowledge. How is this type of knowledge possible? Schlick first considers Kant's answer to this question—i.e. that geometry is the pure form of intuition—but he dismissed this possibility on the grounds that "recent inquires into the principles of mechanics…make it appear doubtful" (*ibid.*, p.84).

Schlick had in mind here the work of Hilbert, Russell and Couturat; Hilbert's axiomatisation of Euclidean geometry being particularly influential. Schlick took the lesson from Hilbert that mathematical objects are defined by a series of implicit definitions, that is, by relations between mathematical concepts. Truth in mathematics, for Schlick, depended solely on the consistency of this logical structure.[43]

However, Schlick did not think that it was only mathematics and logic that were capable of providing truth in the universal sense that he required: the physical sciences, he claimed, contain laws "which are just as exact and immutable as, say, the principle of [non-] contradiction"[44] (*ibid.*, p.85). How were physical truths to be characterised as being capable of verifying themselves as surely as mathematical truths?

Reichenbach draws a distinction between mathematical and physical truth in a manner that is very similar to that we have seen Schlick draw in his (1978a):[45]

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[43] "[For] apodeictically valid demonstrations it is not their intuitive properties which need to come into consideration, but 'only the *relations* between geometrical concepts laid down in the principles of definitions employed' [here, Schlick quoted from (Pasch, 1882, p.98)]" (Schlick, 1978a, pp.84-5)

[44] Kant refers to the "principle of contradiction", however I refer to the "principle of non-contradiction" throughout as this is the accepted contemporary terminology.

[45] And, indeed, Reichenbach explicitly aligns his understanding of the distinction between mathematical and physical truth with Schlick's: "Hilbert's proposition is not an *exhaustive* definition; it is made complete by the totality of axioms…This peculiar mutuality of mathematical definitions, in which one concept always defines another without the need of referring to "absolute definitions," has been clearly stated by Schlick in the theory of implicit definitions…Under these circumstances it is not surprising that mathematical propositions are absolutely certain." (pp.35-6)

> The truth of mathematical propositions depends upon internal relations among their terms; the truth of physical propositions, on the other hand, depends on relations to something external, on a connection with experience. (Reichenbach, 1965, p.34)

Reichenbach understood this difference as stemming from the fact that the objects of knowledge are different for the two types of science: mathematical sciences have as their object objects that are determined by sets of axioms and definitions, whereas physical knowledge seeks to describe objects of experience.

So, the crucial feature of physical knowledge to capture is the sense that the laws of physics—which Reichenbach understood as systems of mathematical relations—are "*true for reality*" (*ibid.*, p.36). Reichenbach claimed that the relation between the laws of physics and reality must be conceived of as a *coordination*: this meant both that the totality of real things must be coordinated to the total system of equations and that each physical individual must be coordinated to individual equations (p.37). What this means precisely is somewhat opaque; Reichenbach offers a couple of examples that seek to clarify the matter. So, Reichenbach suggested that we should view the statement that "the Earth is a sphere" as the coordination of the mathematical figure of a sphere to visual and tactile perceptions of the Earth. He also suggested that the ideal gas law be understood as coordinating the equation $pV = RT$ with direct perceptions of gases (e.g., the feeling of air on skin) and indirect perceptions of gases (e.g., readings on a pressure gauge) (*ibid.*).[46] I think it is clear how indirect perceptions can be coordinated with an equation like the ideal gas law: we can measure the variables with a pressure gauge, gas cylinder and thermometer and check that relationships between these numerical values correspond to those predicted by the ideal gas law. Physical knowledge, then, for Reichenbach, depended upon establishing a relation between external reality and concepts (with the senses mediating between the two).

Reichenbach immediately saw a problem with treating physical knowledge as a coordination between reality and concepts: in paradigm cases of coordination both sides of the coordination are well defined, this is not so in the case of physical coordination. So, consider mathematical coordination whereby one coordinates a discrete subset to a continuum: e.g., the coordination of rational fractions to a continuum. In this case both the continuum and the rational fractions are implicitly defined and known: the coordination consists in selecting the points of the continuum that correspond to the rational fractions. Mathematical coordinations like this provide us with knowledge, claimed Reichenbach,

[46] Reichenbach refers to $pV=RT$ as Boyle's law. I follow modern terminology in referring to this law as the ideal gas law.

because they are *unique*.[47] This is a vital component of Reichenbach's account of coordination: for a coordination to be true it must be unique.[48]

Even in the case of a mathematical coordination, it is not quite this straightforward to find a unique coordination. This is because the problem has not yet been precisely defined since there are an infinite number of ways to carry out the coordination (*ibid.*, pp.38-9). By this Reichenbach meant that when one side of the coordination is a continuum, there is no unique way to pick out a segment of the line that counts as a unit length. So, as the segment of the line chosen to be the unit length changes, so too does the part of the continuum that the rational fractions are coordinated to. Furthermore a continuum does not have a direction attached to it that tells us which way is the direction of increasing magnitude: so should one coordinate the fractions as they increase in size from right to left or from left to right? This problem was to be solved by the specification of additional conditions: that is, one would have to specify the unit length and the direction of increasing magnitude before a unique coordination could be carried out. The coordination is unique when all the necessary additional specifications have been found.

The difficulty in finding a unique coordination is even greater in the case of physical coordination. While in the case of a mathematical coordination certain additional conditions need to be identified before a unique coordination can be found, this is made easier by the fact that both sides of the coordination are implicitly defined. In the case of a physical coordination, this is not the case: a physical coordination is an attempt to identify physical concepts with mathematical concepts, but the physical side is "completely

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[47] This emphasis on a coordination needing to be unique in order to provide knowledge is derived from Schlick. The roots of this claim lie in Schlick's (1978a) claim that for a judgment to be true it must be universal. Schlick's position has evolved somewhat by the time of his (1985 [1918]): here Schlick made a distinction between knowledge and truth, claiming that knowledge required more than just truth: "Truth requires nothing but uniqueness of coordination; as far as truth is concerned, it does not matter what sign is used for that purpose. Knowledge, on the other hand, means unique coordination with the help of certain definite symbols, namely those that have found application elsewhere. If a physicist were to discover a new kind of rays and to name them Y-rays, then the judgment "The rays discovered by the physicist are Y-rays" would of course be true. But this would not mean any advance in knowledge, since the new object would have been designated simply by the use of a new word" (1985, p.66). A unique coordination, then, is enough for truth, but it does not tell us very much. For example one could identify each individual that one comes across with a new name: each coordination is unique—and therefore true—but this is not enough for knowledge. A cognitive judgment requires a novel combination of old concepts, however these old concepts will have been involved in priori judgments and so the new judgment enters a system of judgments. And "By virtue of the interconnection of judgments a new truth receives a specific place in the circle of truths; the fact corresponding to this new truth is thereby assigned to the place that, by virtue of the interconnection of facts, it occupies in the domain of reality. And it is precisely because a judgment points this place out to us that the object or fact becomes *known*. Hence it is the structural connectedness of our system of judgments that produces the unique coordination and conditions of its truth" (*ibid.*, p.67). So, for Schlick, we have knowledge of an object when it is uniquely coordinated to its place in the system of judgments. As we will see, this has distinct echoes in Reichenbach's account of coordination. Reichenbach (1965, p.43) explicitly acknowledges that he takes truth to be a unique coordination in Schlick's sense.

[48] This feature of Reichenbach's account of coordination is also emphasised in (Padovani, 2011, p.50). Padovani translates the original German "*Eindeutigkeit*" as "univocality", I have followed Maria Reichenbach's (1965) translation.

undefined" (1965, p.40). However, there is a "peculiarity" about physical coordination that provides a way of solving the problem:

> There remains the peculiarity that the defined side does not carry its justification within itself; its structure is determined from outside. Although there is a coordination to undefined elements, it is restricted, not arbitrary. This restriction is called the "determination of knowledge by experience." We notice the strange fact that it is the defined side that determines the individual things of the undefined side, and that, vice versa, it is the undefined side that prescribes the order of the defined side. *The existence of reality is expressed in this mutuality of coordination.* (p.42)

In the case of the mathematical coordination of the set of rational fractions to a continuum, the direction of increasing magnitude needed to be set down as an additional condition. In the case of physical coordination, the order of the mathematical side is determined by the physical side of the coordination while the "individual things" of the physical side are determined by the mathematical side. This means that the mathematical side provides concepts under which the physical side can be ordered—e.g. temperature, pressure, mass etc.—while the physical side is used to determine the specific numerical values of these concepts in any given case.

How do we determine the correct coordination in the physical case? As we have seen, the important condition for the truth of a coordination is that it be unique: a coordination is said to be unique when "a physical variable of state is represented by the *same value* resulting from *different empirical data*" (p.45). Reichenbach illustrated what he meant by this with an example from relativity. Einstein's theory predicts that we should measure a deflection of 1.7" of light around the Sun; what would we say if, when it was tested, the measurement was 10"? Here Reichenbach appeals to the theory-ladenness of observation to suggest that in both the case of the 1.7" measurement and the 10" measurement we would have two different answers derived on the basis of empirical data and theory.[49] In the case of the predicted measurement, the empirical data goes in early on in the development of the theory; in the case of the actual measurement, theory is used in the construction of complex measuring instruments. For Reichenbach a coordination is unique when the same value is reached by both methods.

Now we are in a position to make sense of Reichenbach's version of the transcendental question "how is natural science possible?": for Reichenbach, the important

[49] Cassirer, who had taught Reichenbach, also emphasised the theory-ladenness of observation: as we shall see in §3.3 this played a crucial role in his argument in *Substance and Function* (1923a [1910]).

question is "how is it possible to achieve such a coordination in a unique fashion?" (p.46). It is in answering this question that Reichenbach appeals to the "axioms of coordination". Reichenbach introduced these as follows:

> Although [coordinating principles] are prescriptions for the conceptual side of the coordination and may precede it as *axioms of coordination*, they differ from those principles generally called axioms of physics. The individual laws of physics can be combined into a deductive system so that all of them appear as consequences of a small number of fundamental equations. These fundamental equations still contain special mathematical operations; thus Einstein's equations of gravitation indicate the special mathematical relation of the physical variable R_{ik} to the physical variables T_{ik} and g_{ik} . We shall call them, therefore, *axioms of connection*. The axioms of coordination differ from them in that they do not connect certain variables of state with others but contain general rules according to which connections take place. In the equations of gravitation, the axioms of arithmetic are presupposed as rules of connection and are therefore coordinating principles of physics.[50] (1965, p.54)

The axioms of coordination, then, are those principles that act as prescriptions for the conceptual side of a coordination: that is, they are assumptions that serve to determine the physical side of the coordination so that a unique coordination can be found. These are contrasted with the axioms of connection which serve only to describe the relationships between physical variables. Axioms of connection, then, describe empirical relationships between physical variables: they include Einstein's field equations, Maxwell's equations, Newton's law of gravitation etc. The axioms of coordination are those principles that permit a unique physical coordination so that these empirical laws can be stated at all. That is, axioms of coordination are those assumptions that are additional assumptions that are required in order to determine the physical side of a physical coordination.

Immediately after introducing the axioms of coordination as above, Reichenbach gives an example of how one particular axiom of coordination—genidentity, i.e., the idea that an object remains identical with itself throughout time—functions.[51] He considers the

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[50] The original manuscript , available at the Pittsburgh Archives, contains a marginal note attached to this passage that provides an alternative specification of the axioms of connection and coordination: The specific laws can be combined into a deductive system so that all of them appear as consequences of a few fundamental equations. We will call these equations *axioms of connection* because they express the connection between the specific physical magnitudes. Opposite to these are the *axioms of coordination*, which represent the properties of all bodies, reduced to a minimum of propositions. An example of coordinating axioms of old physics are the axioms of geometry; Maxwell's equations are an example of connecting axioms. (Translated by Padovani, 2011, p.55).

[51] "when we speak of the path of an electron, we must think of the electron as a thing remaining identical with itself; that is, we must make use of the principle of genidentity as a constitutive category. This

path of an electron and claims that we can only interpret this as a single moving entity if we presuppose the idea of persisting objects. That is, the principle of genidentity serves to condition the otherwise unconditioned physical side of the coordination, and, in so doing, permits a unique coordination. It is, then, quite clear why Reichenbach understood genidentity to be a constitutive axiom of coordination: it was a necessary assumption in order to have persisting physical objects that the empirical laws could describe. Other axioms of coordination that he mentions—space, time and the principle of probability (p.53)—seem to be similarly fundamental in constituting objects. They all refer to the physical side of a coordination and are understood as, in a sense, defining what is real so as to permit a unique physical coordination.

Reichenbach, as we have seen, understood his axioms of coordination as relativizing Kant's synthetic a priori; however, it seems clear that the axioms of coordination are better understood as analytic principles.[52] The problem that Reichenbach was addressing in *The Theory of Relativity and A Priori Knowledge*, was essentially the same problem that had concerned Schlick: how can a unique coordination between implicitly defined mathematical structure and completely undefined experience be possible? Reichenbach understood this to be how the transcendental question should be formulated in light of mathematical developments since Kant.[53] The axioms of coordination are then appealed to in order to limit experience so as to permit a unique coordination.

Reichenbach understood the experiential side of a physical coordination to correspond to the Kantian faculty of sensibility, and the axioms of coordination were taken to be assumptions about how experience was to be constituted. The problem, from the perspective of seeking to defend the syntheticity of the axioms of coordination, was that the axioms of coordination are treated in a manner analogous to the conditions that are needed to limit one side of a mathematical coordination—such as where on a continuum to place "zero"—which seem to be more clearly matters of convention. This meant that Reichenbach never managed to fully articulate a sense in which his analogue of Kant's faculty of sensibility had any great significance or independence. As such, and given that his version of the transcendental question was based upon Schlick's account of knowledge, it is plain to see how Reichenbach could have come to see his axioms of coordination instead

connection between the conceptual category and the experience of coordination remains an ultimate, not as an analysable residue. But this connection clearly defines a class of principles that precede the most general laws of connection as presuppositions of knowledge though they hold as conceptual formulas only for the conceptual side of the coordination. These principles are so important because they define the otherwise completely undefined problem of the cognitive coordination." (Reichenbach, 1965, p.55)

[52] This was precisely Schlick's (1978b, p.333) objection to Reichenbach's account.

[53] Reichenbach is quite explicit in this respect: see (1965, p.46).

as conventional coordinative definitions which are directly analogous to those additional assumptions needed to secure the uniqueness of a mathematical coordination.[54]

Reichenbach's work, though, is not the extent of the proto-logical empiricist interest in the relativized a priori: Carnap, Friedman claims, sought to build upon and generalise Reichenbach's insight. Carnap's work is interesting, from the perspective of seeking an answer to CC, because ultimately Carnap offers an analytic account of constitutive principles: Friedman's work on Carnap should, then, be read as arguing that the attempt to provide an analytic, non-Kantian, account of constitutive principles is a doomed project. This, for Friedman, means that it is essential to preserve a *synthetic* notion of the relativized a priori. Ultimately, as I will show in §4.1.3, Friedman's argument to this end does not succeed. That is, Friedman seeks to show that the attempt to develop an account of science that secured a role for constitutive principles failed only because Reichenbach's synthetic understanding of the relativized a priori was abandoned in favour of the doomed analytic understanding of constitutive principles. From the discussion of Reichenbach's work, it should be clear that in interpreting Reichenbach's relativized a priori analytically, Carnap was simply accepting that Reichenbach's framework could not support a notion of the synthetic a priori. Let us now turn our attention, though, to the other part of Friedman's argument: i.e., that Carnap's project fails because it provides an analytic interpretation of constitutive principles.

In what sense, then, does Carnap seek to salvage the relativized a priori? Carnap first developed his philosophy of linguistic frameworks in his *Logical Syntax of Language* (2000 [1934]). His central argument was that all standards of "correctness", "validity" and "truth" are meaningful only with respect to the logical rules of a given linguistic framework. As such, it does not make any sense to ask whether a particular set of logical rules is correct or valid; there is no linguistic framework in place that would allow us to judge such a claim. This, says Friedman, means that logical rules are "*constitutive* of the concepts of "validity" and "correctness"—relative to one or another choice of linguistic framework, of course—and are in this sense a priori rather than empirical" (2001, p.31).

Friedman suggests that Carnap's L-Rules can be interpreted as an explication of Reichenbach's understanding of the relativized a priori:

[54] This is ultimately, I think, how Friedman would diagnose the failure of Reichenbach's relativized a priori: in his (2012), Friedman certainly stresses that the main problem is that Reichenbach's account of what is given in sensibility is not given a "sufficiently independent a priori structure". Furthermore, in Friedman's more recent account he seeks to defend an independent faculty of sensibility while moving away from construing the transcendental problem as one of mapping abstract mathematics to experience. See §1.4.2.

> I suggest that Carnap's L-rules or analytic sentences can be profitably viewed as a precise explication of Reichenbach's notion of the constitutive or relative a priori. And in this connection, it is especially interesting to note that Carnap's L-rules include not only pure mathematics but also, in the context of some linguistic frameworks, principles of physical geometry. (Friedman, 1999, p.69)

L-rules, for Carnap, form a logico-linguistic framework that is—as Friedman (2001, p.43) points out—closely analogous to Kuhn's later understanding of paradigms. Friedman's account of the failure of Carnap's research programme ultimately hinges on his reading of Carnap as advocating a variant of naturalism.

This is, of course, not naturalism in the sense that Carnap sought to apply empirical reasoning to the logic of science: Carnap's methodology was, however, naturalistic in the sense that he understood philosophy of science as a branch of science.[55] Carnap was part of the "left-wing" of the Vienna Circle, along with Neurath and Frank among others. For this section of the Vienna Circle, the logic of science was just the a priori part of a broader meta-theory of science. The role of philosophy—as part of this meta-theory—was understood by Neurath as the study of the "behaviouristics of scholars" and by Frank as the "pragmatics of science". On this understanding of philosophy of science, it is not an independent field but, instead, just plays a role in providing the logic and "pragmatics" of science.

Now, as we have seen in §1.2, this is clearly not a conception of the role of philosophy that Friedman can accept: philosophy has a vitally important role as an arena for discussing crucial matters of interpretation in a theory-neutral environment. So his argument against Carnap is that by attempting to frame the constitutive a priori in these terms—i.e. as part of the logic of science that, with the pragmatics of science, is just a branch of science—he cannot possibly hope to fulfil the promise of the relativized a priori. That is, it is the error of treating philosophy just as a branch of science that leads Carnap to adopt a purely formal understanding of the a priori: i.e. there is no room for a synthetic a priori because on Carnap's understanding of the a priori, statements cannot be considered cognitively significant if they are not testable. As we will see in the next section, the idea that the synthetic, relativized a priori is not testable is a central feature of Friedman's (2001) account of constitutive principles.

[55] Uebel (2012, p.9) clarifies this point.

1.3.1.2. The relativized a priori in the Dynamics of Reason

How, then, does Friedman recommend—in his (2001)—reconfiguring the original Reichenbachian conception of the relativized a priori in order to answer CC and maintain the syntheticity of his relativized a priori? Friedman accepts Schlick's distinction between an uninterpreted mathematical framework and an interpreted external reality. As such he divides constitutive principles into two types: mathematical principles and coordinating principles. Mathematical principles are those that implicitly define a mathematical framework: e.g. the axioms of Euclidean geometry define the mathematical framework within which Newton constructed his theory of gravitation. Coordinating principles are akin to Reichenbach's axioms of coordination; they serve to map physical reality to the uninterpreted mathematical framework. Hence:

> [It] is clear that the mathematical parts of our theories, considered independently of the empirical application in question, is in no way empirically tested by such a procedure [as, e.g., observation of the advance of the perihelion of Mercury]; what is empirically tested is rather the particular coordination or correspondence in virtue of which some or another mathematical structure is used to formulate precise empirical laws about some or another physical phenomena. (Friedman, 2001, p.80)

Here, Friedman appropriates Reichenbachian terminology to claim that what is tested is the *coordination* of the mathematical part of our theories with observational phenomena. This coordination is to be carried out by *coordinating principles*. These are defined in the following fashion:

> Their peculiar function is precisely to mediate between abstract mathematical representations and the concrete empirical phenomena these abstract mathematical representations are intended to describe. As such, they do in fact fulfil the characteristically constitutive function first delimited by Kant, and, accordingly they have a genuine claim to be considered as constitutively a priori. (2001, p.77)

Friedman initially uses Newton's laws of motion as an example of this sort of principle. Newton's theory of gravity has, as its foundation, a mathematical theory: Euclidean geometry. On the other hand there are empirical phenomena which one wishes to describe using the purely abstract mathematical theory (Friedman gives the example of the relative motions in the solar system). Coordinating principles are intended to ensure the applicability of mathematical theory to the observed world: in Newtonian physics, for example, the laws of motion act as the principles that coordinate the abstract mathematical foundations of the theory with the empirical laws.

Coordinating principles fulfil this role by containing a minimal degree of empirical content plus some additional information about how this relates to the mathematical part of the theory. Friedman is quite clear that coordinating principles have empirical content and, indeed, can even be falsified:

> It must certainly be acknowledged, at the outset, that the principles in question do have empirical content. If the Eötvös experiments had detected a difference in the accelerations due to gravity in different materials, the principle of equivalence could not simultaneously be maintained. (2001, pp.86-7)

However, if a putative coordinating principle is to successfully connect mathematical and empirical parts of a theory it is surely not enough that it contains only an empirical part: it must also say something about how we are to apply the mathematical part of a theory to the physical world. Friedman does not, as far as I'm aware, claim this himself, but this additional claim is clearly implicit in his conception of coordinating principles. So in the case of, for example, Newtonian physics the mathematical part of the theory contains the concept of straight lines. The law of inertia is taken to be a coordinating principle because it links this aspect of mathematical structure with empirical observations by defining these straight lines as the paths that force-free bodies follow.

So, Friedman answers CC here by arguing that the constitutive principles initially have empirical content. In being elevated to the status of a principle, though, they become more than just an empirical generalisation: they tell us how to coordinate a feature of the physical world with aspects of mathematical structure. It is in this sense that Friedman takes constitutive principles to be unfalsifiable: once one has accepted a certain mapping all experiments are carried out within a framework that takes that mapping for granted.

The foregoing, then, is how Friedman initially presents the relativised a priori. The empirical side of a scientific theory is to be understood as constituted by a purely mathematical part and a part that tells us how to connect the mathematics with empirical observations. In the next section I argue that Friedman's account of the relativized a priori—like Reichenbach's—fails to secure the syntheticity of the a priori.

1.3.1.3. *Syntheticity lost: the thinness of physical coordinating principles*

I think it is now clear, though, that Friedman cannot—and, indeed, no longer wishes to[56]—tie his understanding of constitutive principles too closely to Reichenbach's. There are two reasons for this:

(1) Reichenbach's understanding of the relativized a priori—especially in the role that he sets aside for coordinating principles—is too closely associated with Hilbert's understanding of mathematics as an uninterpreted axiomatic system. This is not a view of the relationship between mathematics and physics that seems plausible from a modern perspective.
(2) It is not at all clear that even if we accept Friedman's initial understanding of the relativized a priori we have sufficient resources to answer CC.

First, let us clarify the reasons for doubting Hilbert's account of mathematics as an uninterpreted mathematical system. The main problem here is that Klein's group-theoretic approach to geometry proved vastly more useful in the development of relativity after Einstein.[57] In particular the axiomatic approach entirely lacks the resources to makes sense of subsequent developments in general relativity that were pioneered by Levi-Civita, Weyl and Cartan.[58]

The second problem is derived from Uebel's (2012) argument that Friedman's (2001) notion of the relativized a priori is better understood as analytic than synthetic.[59] We have seen that Friedman, in effect, argues that when we use coordinating principles to provide the physical meaning of mathematical formalism we take a step beyond Carnap and bestow syntheticity on our constitutive principles. Uebel argues, though, that this is actually better understood in analytic terms: Friedman, in arguing for the ultimate

[56] That Friedman is aware of the problems associated with his view is clear in his (2010a) and (2012). Friedman points to Ryckman's (2005) as convincing him that his (2001) conception of the a priori was too closely tied to Reichenbach's understanding of mathematics (see Friedman, 2010a, pp.697-8). Friedman (2012, p.53n) also suggests that he now doubts that Reichenbach's understanding of the a priori is sufficiently rich to avoid descending into mere analyticity.
[57] Friedman (2010a, p.698) indicates that he is now aware of this, and, consequently, seeks to distance his account from Hilbert's view of mathematics.
[58] See (Ryckman, 2005) and (Stachel, 2007b) for accounts of these developments and the role of group-theoretic geometry.
[59] (Suarez, 2012, p.5) and Uebel himself (pp.15-6) both point out that it is a quite delicate matter to work out what precisely is meant by analytic and synthetic in relation to Carnap's philosophy. The difficulty arises just because Carnap's logic is so different to Kant's. However, I think if we seek to maintain the distinction as an analytic judgment meaning that which is given by logic and which can be known independent of experience whereas a synthetic judgment does require some appeal to experience, the distinction is clear enough to be sensible.

syntheticity of constitutive principles, ignores an important continuity between Schlick and Carnap (p.17).

The relevant aspect of Carnap's account is in his *Logical Syntax*, §51. He argues that while the types of principle that Friedman draws attention to—that seem to have an empirical part—may not look analytic they are, in fact, *framework analytic*. This is distinguished from *definitional analyticity* in the following fashion. In *Logical Syntax* Carnap recommended writing empirical laws into logico-linguistic frameworks as P-rules. P-rules are "extra-logical rules of transformation"; L-rules are "logical rules of transformation". Both L-rules and P-rules are understood as being constitutive of a linguistic framework and as being analytically true with respect to that framework. This means that even though P-rules have an empirical basis, they function as framework principles for a language and—as such—cannot be empirically refuted within that framework.

From here, it should be clear why Friedman's (2001) account cannot avoid this form of analyticity. P-rules are not definitionally analytic: they come about through observation. However they *are* framework analytic: they are accepted as linguistic conventions and—within that language—are true as a matter of definition. This, for Carnap, is sufficient (by the time he came to write *Logical Syntax*) because, following Schlick, he has abandoned the idea that constitutive principles need to be constitutive of objects: they just need to be constitutive of linguistic frameworks.

Uebel's argument is sufficient to show that Friedman's (2001) does not provide a satisfactory Kantian answer to CC: once one has accepted Schlick's account of the relationship between physics and mathematics, one is driven—as Carnap showed—to treat empirical principles as framework analytic. Friedman now accepts this,[60] and argues that if we are to secure the syntheticity of the relativized a priori, we must abandon the understanding of constitutive principles as coordinating mathematical formalism with physical experience. Friedman instead seeks to answer CC by historicising the relativized a priori.

1.3.2. A more radical reconfiguration: a transcendental role for history

In §1.3.1 I showed that the version of the relativized a priori that Friedman defended in his (2001) is insufficient to allow a convincing Kantian answer to CC. Friedman has indicated in his recent works that he is aware of this difficulty: in his (2010a) he accepts that he was mistaken in his (2001) to tie his account of the relativized a priori so closely to Reichenbach and Schlick's understanding of the relationship between mathematics and experience. He

[60] See (Friedman, 2012)

suggests that we should, instead, focus on the group-theoretic account of the relationship between mathematics and experience that is advanced in Klein's work. There are two approaches that we might take to incorporating this key insight into a Kantian account of contemporary philosophy of science.

I would argue that the most natural way to incorporate group-theoretic insights into a Kantian philosophy of science is that which was pursued by Cassirer in his regulative reading of Kant. For Cassirer the invariant properties of a group define the concept of objectivity at a time: this is the starting point for a transcendental analysis of science. Cassirer then, sought to explain the possibility of shared scientific objects-of-experience in terms of the concept objectivity, interpreted in a group theoretic fashion.[61] This, however, is not an approach that Friedman is willing to take. Friedman is reluctant to adopt Cassirer's account of constitutivity because doing so necessitates the rejection of an independent faculty of sensibility.

Friedman argues that this has an important consequence: without the distinction between sensibility and understanding it is impossible to distinguish between regulative and constitutive principles.[62] This is because, for Kant, constitutive principles are those that govern the application of the intellectual faculties—understanding and reason—to the distinct faculty of sensibility. Regulative principles are those that govern the operation of the intellectual faculties independently of sensibility. So, if there is no distinction between understanding and sensibility, then it is not clear how we can distinguish between constitutive and regulative principles. For Friedman, then, this means that there is no room for constitutive principles on Cassirer's Kantianism: as such, Cassirer is in no position to provide an answer to CC and does not provide a meaningfully Kantian account of modern science. This challenge to Cassirer's regulative Kantianism is a significant one: much of the work of the following chapters will be directed towards showing—*contra* Friedman—that a regulative Kantianism can draw a distinction between constitutive and regulative principles.

Friedman, then, insists that if Kantian insights are to be applied to contemporary philosophy we must emphasise the importance of constitutive principles and seek to relativize Kant's *synthetic* a priori. In his (2010a) and (2012), Friedman has started to develop an alternative, historicised, version of the relativized a priori that aims to incorporate a more plausible account of the relationship between mathematics and experience. There are two key aspects to Friedman's new account of constitutive principles: first, Friedman re-interprets Kant's original understanding of the faculty of sensibility in terms of frames of reference and, second, principles are constitutive of a theory in virtue of their historical role

[61] I detail this feature of Cassirer's regulative Kantianism in §3.3.
[62] See (Friedman, 2000, p.117)

in providing a conceptual framework that permits us to apply ideas of space, time and motion to sensibility thus understood.

First let us clarify the manner in which Friedman seeks to re-interpret the faculty of sensibility so as to ground a distinction between constitutive and regulative principles. Friedman suggests that we replace "the Kantian faculty of sensibility with what we now call physical frames of reference—ostensively introduced and empirically given systems of coordinates (spatial and temporal) within which empirical phenomena are to be observed, described and measured" (2012, p.48). So, the faculty of sensibility is replaced by emphasising the idea that our experience of the world is frame-dependant. This means that any observation or measurement that we carry out can only describe the universe from our particular earth-bound perspective.

Friedman (2010a, p.691) characterises the development of space-time theories as the increasingly complex task of describing our frame-dependent observations of the world in a manner that is applicable to all frames of reference. In particular, for Friedman, it is key to integrate laws describing the behaviour of objects from "laboratory frames" on earth with universal laws. The process began with Newton's integration of Galileo's mathematical description of, e.g., falling bodies with the best available description of celestial bodies. Newton achieved this by positing universal laws of motion, from which could be derived a universal law of gravitation. This, of course, gave rise to a new problem of how we should understand the mathematical description of nature to relate to experience: Newton depended on the concept of absolute space, which is not an object of experience.

This, as we see in §2 is the problem that Kant was concerned with in his *Metaphysical Foundations of Natural Science*: i.e., how is it that Newton's mathematical description of nature provides an objective description of sensory experience? Kant took absolute space to be the ideal limit of a series of "relative spaces"[63] that begin with our initial perspective on the universe from earth. We then consider relative spaces that are successively further removed from our particular earth-bound perspective: going from the perspective from the earth via relative spaces that increase in scope through the solar system, galaxy, cluster of galaxies all with Newtonian absolute space serving as the limit of definable relative spaces.[64] This sequence is meant to ground a sense in which our initial experience on earth—which is describable to a very good approximation by Euclidean geometry and Newtonian physics—can "reach" the astronomical frame of reference.

[63] Where relative spaces are understood to be closely analogous to the late nineteenth century idea of inertial frames.
[64] Friedman develops this account in his (1982), (2009) and introduction to (Kant, 2004).

This, as was clarified by Mach,[65] effectively introduced into physics the idea of an inertial frame of reference.[66] Einstein's development of general relativity proceeded by a series of natural extensions of this concept. In special relativity, Einstein put the idea of inertial frames front-and-centre in the guise of the relativity principle. However, this principle was inconsistent with the light postulate. In resolving this tension, Einstein revised the manner in which different inertial frames relate to each other (Friedman, 2010a, p.692). Minkowski, then, used the coordinate transformations that Einstein derived in order to develop a new mathematical framework within which to interpret experience. However, at this stage the mathematical framework could be related to our experience relatively straightforwardly: the new mathematical structure still prioritises the description of objects in inertial frames and these can be connected to our perceptual experiences in a manner analogous to that seen in the case of Newtonian physics.

However, it is far more difficult to incorporate Newton's account of gravitation into Minkowski's mathematical framework: inertial frames of reference are, after all, no longer privileged in general relativity. Here the rotating frame of reference and the equivalence principle are supposed to provide a means to connect the variably curved space-time of general relativity with perceptual experiences of earth-bound frames of reference. In particular, the rotating disk motivates an understanding of the equivalence principle, which replaces Newton's principle of inertia with the claim that force-free objects follow space-time geodesics. While this eliminates the idea of global inertial frames of reference it does permit us to define local frames of reference which can then be connected back to perceptual experience.

So, for Friedman, constitutive principles are those that have played a historical role in permitting us to relate the increasingly abstract mathematical frameworks within which space-time theories have been described back to our ordinary perceptual experience. Our experience is understood as being confined to a particular, ostensively defined frame of reference that stands in for the Kantian faculty of sensibility. Friedman, then, aims to retain a sense in which sensibility is independent while also acknowledging that one must go beyond Kant's description of this faculty as limited to Euclidean geometry and Newtonian physics.

The rotating disk thought experiment is important for Friedman because it provides a way to relate the abstract mathematical structure of general relativity to phenomena observable from earth-bound laboratory frames. This is because the thought

[65] See (DiSalle, 2002b).

[66] The analysis of the development of relativity here is very quick. My intention is just to summarise Friedman's understanding of the role of constitutive principles in the development of the theory: I provide a more detailed discussion of this process in §4.

experiment played a crucial historical role in establishing the possibility of representing gravitation by variably curved four-dimensional space-time in the first place. Once it is clear that it is possible to treat gravitation as space-time curvature, we then have a path by which it is possible to relate abstract mathematics to observable phenomena: i.e. because the four-dimensional space-time of general relativity is infinitesimally Minkowskian. The equivalence principle and the light postulate, then, are constitutive of general relativity on Friedman's account precisely because of their roles in this thought experiment in making four-dimensional space-time a physical possibility.

So, then, the idea is that while we must conduct the empirical parts of our scientific theories from the perspective of laboratory frames on earth there must be a mechanism by which these are able to provide reliable empirical data about the rest of world. Friedman's idea is that rather than treating mathematics as wholly uninterpreted, it should be understood as connected to experience via an analogue of the Kantian schematism: this means that the abstract mathematical structures that we get in general relativity are to be understood via their relation to earth-bound frames of reference which (approximately) have the structure of the Kantian faculty of sensibility. This is how Friedman hopes to avoid the problems that we saw associated with Reichenbach's view in §1.3.1.1:

> But this analogue of the Kantian schematism is also much more substantial than Reichenbachian coordinating principles in so far as the observable phenomena to which the theory is coordinated necessarily have a priori mathematical structure of their own. We thus have a (relativized) a priori mathematical structure at *both* the observational and the theoretical levels, and the two are coordinated with one another by a complex developmental interaction in which each informs the other. (Friedman, 2012, pp.48-9).

So, Friedman hopes to avoid the problem with Reichenbach's understanding of the relativized a priori by arguing that the observable phenomena of a theory have a mathematical structure of their own. So, mathematical and observational levels of a theory have a complex developmental interaction. The mathematical part of a theory is developed through abstracting away from our earth-based observational limitations (towards a regulative ideal), but in order to be tested we must be able to consider the situation from our own frame of reference. Similarly experimental facts revealed on earth can be carried up to higher levels of abstraction and greater invariance.

Friedman's argument in his (2012), then, is that all of our observations are necessarily frame-dependent and that this fact can serve as a modern analogue for Kant's faculty of sensibility. Frame-dependent empirical data is then developed into a global theory by applying our frame-dependent empirical data in reference frames that are far removed from that of our own experience: these more distant frames are connected to our own experience in the sense that they represent successively better approximations of the regulative ideal of absolute space.

In general terms, I think that the above represents a helpful way to understand the development of space-time theories: in particular, it is clear that Einstein was motivated by the desire to strip inertial frames of their privileged status.[67] The problem is that I do not think that this is a promising way to understand constitutivity for two main reasons. First, in appealing to the regulative ideal of absolute space in order to ground the possibility of applying empirical observations to frames of reference that are far removed from our own experience, I think Friedman is mystifying a process that is not, in fact, very mysterious. The only assumption that is required in order to justify the idea that local observations have global application is the idea that the universe has a uniform nature.[68]

Second, I do not see any particularly strong sense in which the idea of ostensively defined frames of reference is analogous to the Kantian faculty of sensibility. The important feature of the faculty of sensibility, from the perspective of understanding constitutivity, is that it passively receives the manifold of intuition and does not interpret it: interpreting the manifold of intuition is the task of the understanding and the constitutive principles associated with it. On Friedman's new account, observable phenomena come with a mathematical structure that must be connected, via historicised constitutive principles, with theoretical mathematical structures. It is the idea of an uninterpreted manifold of intuition that is threatened by developments in logic and Cassirer's Kantianism. It is not clear how the account that Friedman presents salvages this aspect of Kant's faculty of sensibility. Friedman seeks to avoid the problem faced by Reichenbach's account by arguing that observations come in a mathematical structure of their own, which must then be coordinated with the theoretical mathematical structures.

However, while I do not accept Friedman's attempt to reinterpret the Kantian faculty of sensibility in terms of frames of reference, there is another aspect of Friedman's answer to CC that is more promising. Friedman argues that principles are constitutive of

[67] I return to this in §4.

[68] Additionally, I do not think that this amounts to a natural extension of Kant's understanding of the regulative role of absolute space. Kant's analysis of absolute space was intended to explain only why a physics that was built upon the assumption that motion is change of position with respect to absolute space—when there is no absolute space—could be objective. The interpretation of absolute space as a regulative ideal was intended only to secure the objectivity of the idea: I do not see any evidence to suggest that Kant thought that the regulative idea of absolute space was necessary so as to apply earth-bound observations to, e.g., the frame of reference of the Sun.

the empirical side of a theory insofar as they play a role in this historical interaction between mathematical and physical parts of a theory.

This idea is the hallmark of Friedman's historicised conception of the a priori as he presents it in his (2010a):

> In my reconceived version of transcendental philosophy…integrated intellectual history of both the exact sciences and scientific philosophy (a kind of "synthetic history") takes over the role of Kant's original synthetic method; and in particular constructive historical investigation of precisely this kind replaces Kant's original transcendental faculty psychology. (2010a, p.702)

This is, perhaps, most straightforwardly clarified by means of a concrete example. In his comparison of his own position with DiSalle's, Friedman emphasises again the importance that the rotating frame plays in his system. It is through this thought experiment, in particular, that the equivalence principle and definition of simultaneity gain their constitutive status. Friedman makes this clear in the following passage (this is important, so I quote it at length):

> In the case of DiSalle's…treatment of the principle of equivalence, however, we find a particularly clear and direct contrast between his approach and my own. For my approach gives a quite central place to Einstein's example of a uniformly rotating frame (and the resulting non-Euclidean spatial geometry), while DiSalle suggests that Einstein's use of this is at best heuristic since it cannot actually warrant the four-dimensional geometrical structure employed in the finished theory. This is perfectly correct—and, indeed, from the point of view of the finished theory, Einstein's example of the uniformly rotating frame reveals no true space-time curvature in any case, for it arises in precisely the context of a *flat* Minkowski space-time. Yet what DiSalle's account does not satisfactorily explain, in my view, is how the idea of a four-dimensional space-time geometry became a real physical possibility in the first place, and, more generally, it seems to me that DiSalle does not adequately capture how difficult it was to arrive at this idea. The question whether a genuinely physical use of four-dimensional space-time is even possible is prior, in my view, to the question of its warranted correctness, and I claim that it was Einstein's use of the rotating frame, in particular, which first made such a four-dimensional geometry physically possible. (Friedman, 2010, p.725)

Here, Friedman suggests that the equivalence principle is constitutive of general relativity in the sense that it makes a variably curved four-dimensional space-time a physical possibility through its role in the rotating frame thought experiment.

I introduced the rotating frame in §1.2.2: the general idea here is that spatial geometry in a rotating frame is measured as non-Euclidean on the grounds that rigid rods contract if placed in the direction of motion and do not contract if they are placed orthogonally to the axis of rotation. The role of the equivalence principle was to allow Einstein to treat the surface geometry of space-time to be four-dimensional and curved in the presence of a gravitational field. By the time that the rotating frame played a role in Einstein's thought he had come to understand it as the claim that gravitation and inertia are two aspects of the one, inertio-gravitational, field. So, Einstein understood that inertial effects—such as the centrifugal force—could be transformed away and replaced by a suitable gravitational field. How does the equivalence principle, then, make four-dimensional space-time a real physical possibility? The most mathematically simple way to transform the situation from one of rotation to one in which the centrifugal forces have been replaced by gravitational forces—once one also adds in time-dilated clocks—is to appeal to a four-dimensional generalisation of Gauss's theory of surfaces.[69] So, together, physical geometry, Lorentz contraction (and the definition of simultaneity) and the equivalence principle—through their role in the rotating frame thought experiment—allowed Einstein to treat four-dimensional space-time as a real physical possibility.[70]

In his (2010a), then, Friedman seems to understand constitutive principles as being those that play a historical role in the process by which a purely mathematical possibility became a physical possibility. In his (2001), it was relatively clear what Friedman meant by mathematical and physical possibility. However, Friedman's (2010a) disavowal of Schlick's understanding of the distinction between mathematics and experience means that it is now no longer precisely clear what he means by this. Friedman does though state that he wishes to ground his account of a group-theoretic understanding of mathematics (2010a, p.698)

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[69] Friedman explicitly explains the role of the equivalence principle in terms of mathematical simplicity: see (Friedman, 2010a, p.789, n.302).

[70] It is worth pointing out here that it is not at all clear how plausible it is to argue that these principles together rendered four-dimensional space-time a real physical possibility when the argument eventually hinges upon considerations of mathematical simplicity. I would suggest that there is a clear sense in which the rotating frame thought experiment—without the equivalence principle—renders the idea of non-Euclidean geometry physically possible because it enables us to see how we might actually measure a non-Euclidean geometry. The matter is nothing like as straightforward for explaining the role of the equivalence principle: the equivalence principle certainly can be appealed to in order to motivate the idea that gravitational fields cause spatial geometry to be measured as non-Euclidean, but the additional claim that this necessitates *four-dimensional* variable geometry seems to be based on considerations solely of mathematical simplicity. It is, therefore, not clear that four-dimensional variable geometry is rendered a genuine physical possibility by the rotating frame thought experiment in the same way that the possibility of measuring non-Euclidean surface geometry is rendered physical by consideration of rotation.

and this can provide some insight into how Friedman might understand mathematical and physical possibility. Mathematical concepts, such as four-dimensional space-time, can be applied to physical situations when there is a means of relating those concepts to measurements. In the case of four-dimensional space-time, this requires just that the invariant line element *ds* can be determined through measurements carried out with rods and clocks. So, Einstein became aware that, in virtue of its invariance, the line element was the physically significant aspect of four-dimensional treatments of relativity. Because the line element could be determined by measurements with rigid bodies and clocks, four-dimensional space-time became a physical possibility.

This gives rise to a problem. From the perspective of the contemporary understanding of general relativity, it is not the case that the line element receives its physical significance because it corresponds to measurements with rods and clocks: the line element is epistemically prior to rods and clocks and governs their behaviour.[71] The line element does not need to be granted physical significance through its determination with measuring instruments: it gains physical significance just in virtue of it being frame-invariant. From this emerges the crucial difference between Friedman's approach to constitutivity and the regulative approach. For Friedman, there is a problem as to how mathematics relates to physics; on Cassirer's regulative view, which emphasises the group-theoretic view—mathematics is essentially applicable to experience.[72] From this perspective, Friedman appeals to constitutive principles to solve a problem that need not arise in the first place.

Friedman's current answer to CC, then, can be summarised as follows. In providing an account of the possibility of a scientific theory—and its empirical concepts—we must pay attention to the complex developmental interaction between observational and theoretical parts of the theory. The sensibility—as ostensively defined frames of reference—plays a crucial role here because it is at this level that the observational part of the historical development of a theory takes place. This is then connected to the abstract, mathematical, part of a theory through analogy to Kant's treatment of absolute space as a regulative ideal. Principles are constitutive of the empirical side of a theory insofar as they render crucial empirical concepts a coherent possibility in the first place: this, in the case of the equivalence principle and definition of simultaneity, is achieved by the role that these principles played in the rotating frame thought experiment.

[71] This is essentially the main objection that Ryckman (2005) offers to Friedman's account.
[72] See §3.3 and (Heis, 2011a).

1.4. Constitutive principles: the way forward

Friedman, then, provides us with the following answers to CR and CC:

CR: The development and acceptance of the theory of relativity was rational because Einstein appealed to theory-neutral philosophical ideas that were available to all scientists. So, e.g., the representation of gravitation as space-time curvature was rendered a coherent physical possibility through Einstein's application of Helmholtz's understanding of physical geometry and the equivalence principle.

CC: Constitutive principles are those that played a historical role in making a mathematical possibility—e.g., four-dimensional space-time—a genuinely physical possibility. The syntheticity of the relativized a priori is secured through appeal to the historical interaction between theory and experience in the laboratory frame. Constitutive principles are not merely analytic because they played a historical role in allowing abstract mathematical structures to be related to empirical observations from the perspective of an earth-bound frame of reference.

These are both intriguing and rich Kantian answers to CC and CR. The idea that philosophy can act as a meta-paradigm that provides a common stock of ideas to drive scientific revolutions and render them prospectively rational is innovative and the case-study of Einstein's development of relativity seems to give the idea weight. I think it is also clear that if Friedman's reinterpretation of Kant's faculty of sensibility is plausible, then this blocks any attempt to treat the constitutive a priori as mere conventions. This is especially clear when one recalls Friedman's emphasis on the necessity of each of the developments in question (which is made more plausible than it initially appears when understood in terms of the style of conceptual analysis advanced by DiSalle).

Friedman's historicisation of constitutive principles, I think, contains an important insight that will feature in my own eventual answer to CC. However, I do not think that Friedman's attempt to defend the syntheticity of his constitutive principles is a particularly attractive route for the Kantian to take. In the following chapters, I argue that a Kantianism that places greater emphasis on the role of regulative principles can provide a more plausible contemporary account of science. I argue in the next chapter that in Kant's account of science, the regulative role of reason features much more prominently than it does in Friedman's contemporary Kantianism.

Friedman's rejection of the regulative approach, I argue in chapter 3, is based on Friedman's own characteristic reading of Cassirer: he argues that in abandoning the distinction between understanding and sensibility Cassirer also loses the capacity to make any distinction between constitutive and regulative principles. Now, this, I argue is a misreading of Cassirer: it is quite clear in his (1923) that Cassirer does take some principles to be constitutive and not regulative. On the reading of Cassirer that I offer, the sorts of worries that Friedman has with an approach that draws on Marburg neo-Kantianism are substantially reduced.

I suggest that we can develop a far more appealing contemporary Kantianism if we make use of some of the resources of Cassirer's philosophy. There are two areas in particular that Cassirer's Kantianism seems more relevant to contemporary concerns than one based upon Reichenbach's. First, Cassirer has a more sophisticated philosophy of mathematics and logic than Schlick. Cassirer, in particular, fully appreciated the importance of Klein's group-theoretic approach to geometry; this is a distinct advantage over accounts based on Hilbert's axiomatic approach (especially given Weyl's subsequent development of general relativity along Klein's lines). Second, the regulative reading of Kant as it is developed by Cassirer allows us to develop a Kantian version of structuralism according to which objects are only knowable insofar as they are embedded in structure. This, I suggest, is a form of ontic structuralism: the objects of modern physics do not exist independently of structure and there is a genuine sense in which structure exists independently of individual objects. This, though, does not amount to full blown ontic structural *realism* because (i) structure is understood as a regulative demand of reason and (ii) there remains a sense in which the Kantian is primarily concerned with knowledge of objects—it is just that this is only made possible by regulative structural demands. I develop this view in my discussions of Cassirer (§3.3), the development of general relativity (§4) and ontic structural realism (§5).

A regulative Kantianism, I argue, is better placed to explain the historical development of general relativity and will ultimately provide a more attractive philosophical account of science. I should stress that I do not seek a solely regulative Kantianism—of the kind that Friedman takes Cassirer to seek—I take it that there is a middle ground between Freidman's constitutive Kantianism and Friedman's reading of Cassirer. Constitutive and regulative principles, I suggest, dovetail more than is apparent in Friedman's account of the development of relativity:[73] Friedman's account on its own provides an inadequate account

[73] This is only a criticism that can be levelled against Friedman's account of relativity: in his (1982) account of Kant's philosophy Friedman assigns much greater weight to the regulative function of reason. I suggest that

of the historical development of relativity, while Cassirer's view on its own struggles to provide a satisfactory account of the role of experience in science. A middle way, I suggest, is possible, addresses both these weaknesses and permits the development of a Kantian structuralism. The first step towards reuniting the constitutive and regulative aspects of Kantian philosophy is to provide an account of the roots of these ideas in Kant's own philosophy of science. This will be the task of the next chapter.

in his work on relativity he seeks to minimise the regulative role of reason because he is concerned that this will open the door to a Marburg-style rejection of an independent faculty of sensibility.

2

The role of constitutive and regulative principles in Kant's understanding of the law of gravitation

2.1. Introduction: constitutive and regulative principles

In the previous chapter I outlined Friedman's account of the constitutive a priori. Friedman, as we saw, is keen to emphasise the Kantian roots of this understanding of scientific theories. In broad terms, Friedman argues that Kant understands the laws of motion and Euclidean geometry to be constitutive of the empirical part of Newton's theory in the sense that they provide the conceptual framework within which the law of universal gravitation is enunciated. The task of this chapter is to explore in more depth the sense in which the laws of motion, in particular, are constitutive of the empirical law of gravitation.

I begin the chapter by providing a brief outline of Kant's transcendental philosophy so as to clarify the roles of the different faculties in Kant's overall scheme. Of particular relevance is the role of the understanding and intuition in knowledge, and, more specifically, the role of constitutive principles in governing the application of the understanding to intuition. The notion of regulative principles, which in Kant's philosophy have a crucial role to play in making empirical judgments, is also introduced.

In the *Dynamics of Reason*— and, to a lesser extent, in "Synthetic History Reconsidered"—Friedman presents his understanding of Kant's treatment of Newton as relatively uncontroversial. That this has become something close to a received interpretation of Kant's work is largely a result of Friedman's (1992a). It is in this work that Friedman makes the most compelling case for treating the laws of motion as constitutive of the empirical side of Newton's theory and the matter is not as straightforward as one would expect.

Friedman's work is a response to Buchdahl who, in his (1969) argues that regulative principles play a much more pronounced role in Kant's derivation of the universal law of gravitation than constitutive principles do. In §2.3 I examine two problems, identified by Buchdahl, with taking there to be a strict division between constitutive and regulative principles grounded in the claim that constitutive principles govern the function of the understanding while regulative principles govern the function of reason. First, Kant distinguishes between constitutive and regulative categories of the understanding. Second, Buchdahl claims that empirical causal laws can only be drawn from an already given series of events: this means that the construction of physical laws—e.g., the law of gravitation—must be governed by reason rather than understanding. I argue that the first of these two problems is explained by Kant's drawing a distinction between principles that are constitutive of intuition and principles that are constitutive of experience: in this section I also explain what Kant might mean by this. In the rest of this chapter, I turn my attention towards analysing Kant's understanding of the role of constitutive and regulative principles in establishing the law of gravitation.

My analysis has two steps. First, I consider Buchdahl's more general concern that constitutive principles ultimately play little role in explaining the possibility of pure natural science. As an initial response to this I consider Kant's defence of matter as necessarily possessing an attractive force, which, I suggest may serve as a model for how he intends both constitutive principles and experience to play a role in grounding empirical judgments.

Second I consider the derivation of the universal law of gravitation, which describes the effects of the necessary attractive force of matter. I argue that the problem here is sharper: nevertheless, I will argue that one should not place as much emphasis on the role of regulative principles in deriving the law of universal gravitation as Buchdahl does. Instead one should view, as Friedman does, constitutive and regulative principles as acting together in order to secure the objective validity of pure natural science.

2.2. A sketch of Kant's system of knowledge

Before we can give an account of the role of constitutive principles in establishing the universal law of gravitation, it is helpful to clarify some of the central parts of Kant's system of knowledge. In this section I will provide a brief survey of Kant's transcendental philosophy.

2.2.1. Sensibility, Understanding & Reason

The main transcendental question, which Kant's entire critical system is an attempt to answer, is as to how synthetic a priori judgments are possible. Synthetic judgments can be either a priori or a posteriori, and Kant contrasts them with analytic judgments. Analytic judgments are based entirely on the principle of non-contradiction and say nothing beyond that which is already contained in the concept that is the subject of the judgment. Thus he considers the judgment "Gold is a yellow metal" to be analytic, because the concept of gold is already thought of as containing the knowledge that gold is both yellow and metallic: no further experience is necessary (2010, p.17; 4:267).[74] As such, all analytic judgments are a priori.

Synthetic judgments are those which are not analytic and which involve experience of some form. Certain synthetic judgments, most notably those of mathematics, are taken to be a priori. Classifying mathematical judgments, such as 7+5=12, in this fashion ran contrary to the received wisdom of the time, which—seeing that mathematical inferences proceed via the principle of non-contradiction—took mathematical judgments to be analytic. Now, Kant accepts that mathematical judgments have an a priori *aspect*, but he maintains that analyticity alone is insufficient to explain the possibility of mathematical judgments.

To understand the category of synthetic a priori judgments, let us consider—as Kant does in the *Prolegomenon*—the proposition 7+5=12. While we might initially imagine that the proposition 7+5=12 is purely analytic in that it can be deduced from the concepts of "a sum", "5" and "7" with the principle of non-contradiction, this is in fact not the case. This is because the concepts "5" and "7" have no meaning without recourse to something external to them: Kant suggests that we must come to understand the numbers by, e.g., comparison with the number of fingers on our hands. So while we can be analytically acquainted with the concept "the sum of 7 and 5" we cannot know that this sum is equivalent to the number 12 independently of intuition (2010, p.19; 4:269).

The possibility of synthetic a priori judgments is ultimately explained by a division of the human intellect into the sensibility and the understanding. The sensibility is a purely receptive faculty that presents sensory data, called intuitions, to the human intellect. The understanding on the other hand is an active faculty which provides a priori conditions for knowledge of objects.

Kant's account of perception is given in the subjective deduction of the categories, and it is here that the role of the understanding in experience is explained. The categories

[74] At this point I should include a note to clarify my referencing system for Kant's works. For referencing Kant scholars I use the (author date, page) system, in keeping with the other chapters of the thesis. This, however, is not the standard fashion in which to reference Kant's works, so in referencing Kant I follow the custom of citing from Kant's *Critique of Pure Reason* by the pagination of the first edition of 1781 (A) and/or the second edition of 1787 (B). All citations from Kant's other works are located first by the year of publication and page reference of my preferred translation and then by the volume and page number of *Kant's Gesammelte Schriften, herausgegeben von der Deutschen Akademie der Wissenschaften*. So, here, (2010, p.17) refers to Hatfield's translation of the *Prolegomena* and (4:267) refers to page 267 of volume 4 of the collected works. I have used English translations of Kant's works; my preferred translation for each of Kant's works that I reference is given in the Bibliography. Where the translator has inserted additional text into Kant's work, I have included the translator's initials in the square brackets with their additional text.

are the most general rules of the understanding for synthesising the manifold of sensibility into objects. Kant explains this in the following manner:

> The *a priori* conditions of a possible experience are at the same time conditions of the possibility of the objects of experience. Now I assert that the *categories* are nothing other than the *conditions of thinking in a possible experience*, just as *space* and *time* contain the conditions of *intuition* for the very same thing. They are therefore also fundamental concepts for thinking objects in general for appearance. (A111)

To explain Kant's theory of perceptual synthesis we must also appeal to the synthesis of apprehension, which is defined by Kant in the following way:

> [By] the *synthesis of apprehension* I understand the composition of the manifold in an empirical intuition through which perception, i.e., empirical consciousness of it (as appearance) becomes possible. (B160)

To see how this might work, consider a body observed from a few different angles to illustrate how both the object and the intellect contribute to experiencing the body *as a body*. Now, each image of the object is from a different perspective, but there is nothing perspectival about the object as it is *conceived*. The body appears as an object of experience (as a body) when the manifold of sense, i.e. the images of the body from different perspectives, is taken as a series of images with associated judgments (e.g. 'this book is facing me side-on'). The process of synthesis of apprehension allows one to conceive of the images of the body from different perspectives as successive encounters with *one* object. This synthesis of apprehension arises from two sources: the receptivity of sensibility which provides the images of the intuited items and the spontaneity of understanding which accounts for the object appearing to us as a single object.

This situation is clarified towards the end of the *Critique of Judgment*. Kant asks: what would a non-human intellect be like? How would it think? How would nature appear to it? (2007, p.229; 5:401) Here Kant describes the human intellect as discursive, meaning that human knowledge of the world must proceed through concepts; it requires mediation among concepts according to a rule. So, whereas God's immediate, instantaneous understanding necessitates his intuition of objects as they there are in themselves, human intuition must be sensible. This, Kant emphasises means that: "Human understanding cannot avoid the necessity of drawing a distinction between the possibility and the actuality of things" (*ibid.*).

So for humans an object is actual when it is present in sensible intuition and cognisable under the concepts of the understanding. Sensible intuition alone would not allow us to cognise anything *as an object*, understanding alone would tell us only of the *possibility* of an object. What if this were otherwise? Kant identifies an intuitive understanding as one in which there is no distinction between possibility and actuality. i.e. as one in which every object that the understanding cognised was *actual*. This type of understanding is productive of the reality of the objects that it thinks (and – in the first critique – is suggested as the sort of intellect God might have). This is the first contra-factual conclusion: that a non-human, intuitive understanding, produces in actuality anything that it thinks. Sensibility would have no role to play.

Kant goes on to make a second contra-factual conclusion: that an intuitive understanding would know of no distinction between contingency and necessity:

> An understanding into whose mode of cognition this distinction did not enter would express itself by saying: All objects that I know *are*, that is, exist; and the possibility of some that did not exist, in other words, their contingency supposing them to exist, and, therefore the necessity that would be placed in contradistinction to this contingency, would never enter into the imagination of such a being. (2007, p.230; 5:403)

This means, then, that the concepts of actuality, possibility, necessity and contingency are all only subjectively valid.[75]

From this point Kant is able to argue that a faculty of judgment (or reason) is necessary. The faculty of judgment is defined as follows:

> The faculty of judgment in general is the faculty of thinking the particular as contained under the universal. If the universal (the rule, the principle, the law) is given then the faculty of judgment which subsumes the particular under it…is *determining*. But if only the particular is given, for which the universal is to be found, then the faculty of judgment is merely *reflective*. (2007, p.15; 5:179)

The faculty of judgment, then, can be used in two ways. First, to make *determinative* judgments and, second, to make *reflective* judgments. The distinction between determinative and reflective judgment resembles the distinction between deductive and inductive

[75] Some care is needed here with terminology. At this stage in the *Critique of Judgment* Kant means, I think, something different by subjective validity than the technical term familiar from the first *Critique*: here he is trying to capture the idea that modal concepts are valid only for a being with a discursive intellect. A being with a different sort of intellect will not necessarily have any modal concepts.

reasoning: in a determinative judgment one derives particulars from a universal and in a reflective judgment one seeks to determine a universal from a set of particulars.. In the *Critique of Judgment* Kant is primarily concerned with providing a justification of the use of reflective judgment is natural science. Kant argues that the use of this reflective judgment is merely regulative, as opposed to constitutive as the principles of the understanding are. This means that, while it doesn't provide the conditions for the possibility of experience, it remains a necessary part of the manner in which we interpret experience.[76]

In the *Critique of Pure Reason* emphasis is placed on the role of the related faculty of reason. This operates by regulative ideas—psychology, cosmology and theology—which guide our thoughts of experience but do not constitute experience. It works, then, by the same rules that govern reflective judgment. Kant describes the role of reason as follows:

> Understanding may be regarded as a faculty which secures the unity of appearances by means of rules, and reason as being the faculty which secures the unity of rules of understanding under principles. Accordingly, reason never applies itself directly to experience or to any object, but to understanding, in order to give to the manifold knowledge of the latter an *a priori* unity by means of concepts, a unity which may be called the unity of reason, and which is quite different in kind from any unity that can be accomplished by the understanding. (A302/B359)

Reason, then, demands the systematisation of our knowledge: i.e. it strives for unity. This feature of the faculty of reason should be emphasised for it will play a crucial role in the argument of §2.4.

The above should serve as a clarification of the role of understanding and intuition in experience, on Kant's account, and to give an introduction to what is meant by regulative principles as they are employed in both reflective judgments and reason.

2.2.2. The Analytic of Principles and the division of the Table of Categories

In the previous section I have introduced the central problem that Kant sees himself as addressing in the first Critique—the possibility of synthetic a priori judgments—and the key conceptual tools which he mobilises to try and solve the problem. I provided a sketch

[76] Kant's proof of this is given in §§69-78 of the *Critique of Judgment*. Central to Kant's argument is the regulative idea of the Unconditioned, which itself contains two further ideas: that we must seek unity in nature and that it must be possible to find unity in nature. The search for unity in nature, for Kant, is a necessary feature of the process of human reasoning. The use of reflective judgment is justified on the grounds that its use assumes a unified nature: it is through this relationship to the regulative idea of the Unconditioned that Kant is able to justify the use of reflective judgment as a necessary feature of human interpretation of experience.

of Kant's account of perceptual synthesis, according to which our cognition of objects as objects depends upon an interplay of the intuition and the understanding. However, this is not the end of the matter: Kant is left with the question of how two such seemingly different faculties can be unified in a perceptual act. That is, how is a sensory intuition able to be bought under concepts? This is the problem with which Kant begins the Schematism (the second chapter of the Analytic of Principles) in which Kant treats 'of the sensible condition under which alone the pure concepts of the understanding can be employed' (A136/B175). That is, he argues that the pure concepts of the understanding can only be applied to the sensory manifold once the temporal nature of our intuition is taken into account. The Schematism is particularly pertinent for the project of this chapter for two reasons:

1. It provides a basis for a division of the Table of Categories into mathematical and dynamical categories. This distinction will impact upon our discussion because experience is assigned a greater role in the dynamical categories than in the mathematical categories.
2. The discussion of the Schematism also leads to the central problem of the Analogies of Experience. The Analogies, we will see, have an important role to play in Kant's derivation of his laws of motion.

As such it is worth looking at the Schematism, and how it informs the above issues, in some detail.

The problem of the Schematism arises for Kant because he takes it that for an object to be brought under a concept it must, in some sense, be 'homogeneous' with that concept:

> In all subsumption of an object under a concept the representation of the object must be *homogeneous* with the concept; in other words the concept must contain something which is represented in the object that is to be subsumed under it…Thus the empirical concept of a *plate* is homogeneous with the pure geometrical concept of a circle. The roundness which is thought in the latter is intuited in the former. (A137/B176)

So, in ordinary cases, one is able to subsume an object under a concept if a property of that object coincides with the concept. The special problem in the case of the categories is that the pure concepts of the understanding are quite *heterogeneous* with empirical intuitions. Kant gives the examples of causality, which he claims can never be met with in intuition. He solves this problem by positing some third thing which is homogeneous with both the pure concepts of the understanding and with empirical intuitions:

> Obviously there must be some third thing, which is homogeneous on the one hand with the category and on the other hand with the appearance, and which thus makes application of the former to the latter possible. This mediating representation must be pure, that is, void of all empirical content, and yet at the same time, while it must in one respect be *intellectual*, it must in another be *sensible*. Such a representation is the *transcendental schema*. (A138/B177)

This seems a natural solution to the problem: but it is one that raises a number of questions. First, given the discussion in the previous section, it should be immediately clear that this transcendental schema seems to enjoy quite a strange status: if the understanding and intuition are meant to be entirely separate sources for our knowledge, how can any representation be both intellectual and sensible?

Second, some work is needed to clarify precisely what a schema is. It may seem natural to treat a schema as an image of some sort, but this is a possibility that Kant is quick to rule out. Instead he is keen to characterise a schema as either a *product* of the imagination[77] or as a *representation* of a process of the imagination.[78]

Having a better idea of what a schema is will help us understand how it can have both sensible and intellectual elements, so let us look at that problem first. Kant's account of what a schema is, and what it is not, is as follows:

> The schema is in itself always a product of imagination. Since, however, the synthesis of imagination aims at no special intuition, but only at unity in the determination of sensibility, the schema has to be distinguished from the image. If five points be set alongside one another, thus, ..., I have an image of the number five. But if, on the other hand, I think only a number in general, whether it be five or a hundred, this thought is rather the representation of a method whereby a multiplicity...may be represented in an image in conformity with a certain concept, than the image itself. For with such a number

[77] Kant's account of the role of the imagination in our cognition is somewhat complex: for our present purposes I take it that treating the imagination as a particular incarnation of the understanding is sufficient to understand Kant's intention in the Schematism and does not, I think, lead to any obvious misunderstandings. That Kant understands the imagination to perform the same sort of role as the understanding is made clear in a footnote to the B-edition Transcendental Deduction: 'the synthesis of apprehension, which is empirical, must necessarily be in conformity with the synthesis of apprehension, which is intellectual and contained in the category completely *a priori*. It is one and the same spontaneity, which in the one case, under the title of imagination, and in the other case under the title of the understanding, brings combination into the manifold of intuition' (B162n).

[78] This distinction is emphasised in (Rosenberg 2005, p.145)

> as a thousand the image can hardly be surveyed and compared with the concept. This representation of a universal procedure of imagination in providing an image for a concept, I entitle the schema of the concept. (A140/B179-80)

It is this paragraph that gives rise to the second problem: in the first sentence a schema is considered as a product of the imagination, at the end of the paragraph, Kant describes a schema as a representation of the procedure of the imagination.

The situation may be clarified somewhat by considering Kant's explanation of why a schema cannot just be an image (A140-1/B180), which follows immediately from this paragraph. Here Kant considers triangles and points out that no image can ever capture the concept. This is because, first and foremost, 'It would never attain that universality of the concept which renders it valid of all triangles' (A141/B18). That is, every triangle is right-angled, obtuse or acute and as such no single image can capture what is meant by the general concept 'triangle'. The situation is the same with numbers. Every number can be depicted by the appropriate amount of dots, but this alone does not bring us closer to the concept of 'number' in general. For this, Kant claims, we need a process. So, the idea, presumably, is that the concept 'number' must be understood by something like the process of counting by keeping a tally.

The same is true, Kant says, for empirical concepts such as 'dog':[79]

> The concept 'dog' signifies a rule according to which my imagination can delineate the figure of a four-footed animal in a general manner, without limitation to any determinate figure such as experience, or any possible image that I can represent *in concreto,* actually presents. (A141/B180)

Again we see that concepts are meant to signify a rule according to which individual instances of the concept can be recognised. The role of the imagination here is crucial:

> [The] *image* is a product of the empirical faculty of reproductive imagination; the *schema* of sensible concepts, such as figures in space, is a product and, as it were, a monogram of pure *a priori* imagination, through which, and in accordance with which, images themselves first became possible. These images can be connected with the concept only by means of the schema to which they belong. (A141-2/B181)

[79] Kant does not himself explicitly describe the concept 'dog' as empirical, but I think that viewing it as such gives us a good insight into what is meant by an empirical concept in Kant's work that will prove helpful in the following sections. The concept, I take it, is empirical in the sense that the concept 'dog' can only arise from experience.

What is important here is the manner in which imagination—a function of the understanding—and sensibility combine in the schema. Now, the understanding is a faculty of rules that, through the imagination, constructs concepts as a process. Concepts in this form, though, cannot be applied to experience because they are spatial. As such, the concept itself must have a sensible form: this is what Kant refers to as the "image" in the above passage. In some sense the process of the understanding that defines a concept can be represented spatially by the imagination, which allows the schema to act as the "third thing" homogeneous to both concepts and intuition.

Up to this point, however, we have dealt only with the Schematism as it applies to mathematical and empirical concepts. This is helpful in illustrating the sort of thing that is meant by a schema, but to fully understand the Schematism it is necessary to see how Kant also schematises the categories. The empirical and mathematical concepts that Kant schematised, are schematised in the sense that one must take into account the form of our (outer) intuition. The categories are schematised in a similar fashion, but this time with respect to inner intuition: time.

Kant argues that the categories need to be schematised because:

> Without schemata [the categories] are merely functions of the understanding of concepts; and represent no object. This [objective (NKS)] meaning they acquire from sensibility, which realises the understanding in the very process of restricting it. (A147/B187)

Rosenberg (2005, p.152) suggests understanding this as follows: the pure concepts of the understanding, for Kant, are intended to be the same for all beings (with a receptive faculty of sensibility). It is the specific form of a being's sensibility that determines how the categories must be schematised. Now, for humans, the most fundamental form of sensibility is *time*. As such, the categories are schematised for us by being restricted principles of the intelligible unity of specifically temporal manifolds. Hence:

> The schemata are…*a priori* determinations of time in accordance with rules. These rules relate in the order of the categories to the *time-series* [Quantity], the *time-content* [Quality], the *time-order* [Relation], and lastly to the *scope of time* [Modality] in respect of all possible objects. (A145/B184-5)

The main task of the Schematism is to show that in order for it to be possible to apply the pure concepts of the understanding in synthetic judgments to the objects of possible experience the pure concepts must be considered as the pure concepts of beings whose perception of the world requires the intelligible unity of a temporal manifold. Once schematised:

> The schemata of the pure concepts of the understanding are thus the true and sole conditions under which these concepts obtain relation to objects and so possess *significance*. In the end, therefore, the categories have no other possible employment than the empirical. As the grounds of an a priori necessary unity that has its source in one original apperception, they serve only to subordinate appearances to universal rules of synthesis, and thus to fit them for thoroughgoing connection in one experience. (A146/B185)

This is a slightly quick exposition of the role of the schematised categories in Kant's project: the more detailed discussion of empirical and mathematical concepts, hopefully, informs the discussion of the categories so that the importance of their being schematised according to our form of inner sense is clear.[80] At the beginning of this section I suggested that the Schematism also impacts upon a distinction that Kant draws between mathematical and dynamical categories. This is a central distinction in Kant's work and it impacts on the work of this chapter for two reasons: first, there are sections of Kant's work where he seems to regard the dynamical categories as regulative—which seems odd given that the categories are usually all thought of as constitutive— and, second, Kant emphasises a distinction between mathematical and dynamical conceptions of how matter fills space and this distinction may be informed by the division of the Table of Categories. In broad terms the distinction can be drawn as follows: the mathematical is that which can be known independent of experience, whereas the dynamical—while still capable of being constitutive in a sense—requires some input from experience.

The distinction is first introduced in the *Critique of Pure Reason* in the B-edition version of the Transcendental Deduction, as a division of the table of categories. The categories under the headings of quantity (unity, plurality and totality) and of quality (reality, negation and limitation) are *mathematical*; the categories under the headings of relation (of inherence and subsistence, of causality and dependence, and of community) and of modality (possibility–impossibility, existence–non-existence and necessity–consistence) are *dynamical*. The distinction is drawn as follows:

> [The table of categories] may, in the first instance be divided into two groups; those in the first group being concerned with objects of intuition, pure as well as empirical, those in the

[80] The emphasis here on the importance of temporal unity is a central feature of Kant's Critique, and will play a key role in the discussion of §2.3.2.

second group with the existence of these objects, in their relation either to each other or to the understanding. (B110)

Kant says little more about the division here—noting that it is only in the dynamical categories that we meet pairs of leading and contrasting concepts, e.g. substance vs. accident—and the above comments are somewhat cryptic. The function of the mathematical categories seems clear enough: they apply to both pure and empirical intuitions in the manner that was discussed in §2.2.1. It is less clear what it means for the dynamical categories to apply to the "existence of these objects [of experience]" whether in relation to each other or to the understanding. Some light, though, is shed on this in the System of Principles of Pure Understanding, which immediately follows the Schematism. Having shown in the Schematism that one is justified in applying the pure concepts of the understanding in synthetic judgments, Kant moves in the System of Principles to 'exhibit, in systematic connection, the judgments which the understanding, under this critical provision, actually achieves *a priori*' (A148/B187).

The task of the System of Principles, then, encompasses deriving both the analytic and synthetic a priori judgments that the understanding is compelled to make. Kant, then, begins this task by elucidating the 'highest principle of all analytic judgments': the principle of non-contradiction.[81] For our purposes, though, what Kant says of synthetic a priori judgments is much more interesting. Now, whereas analytic judgments were characterised by pure general logic, synthetic judgments lie in the realm of transcendental logic.[82] Kant here defines the difference as follows:

In the analytic judgment we keep to the given concept, and seek to extract something from it. If it is to be affirmative, I ascribe to it only what is already thought in it. If it is to be negative, I exclude from it only its opposite. But in synthetic judgments I have to advance

[81] My account of the System of Principles here is very brief, I wish only to give a very general sense of what this part of the Critique is intended to achieve before introducing the Table of Principles and its division into mathematical and dynamical principles, which is what I am primarily interested in at this point.
[82] The Analytic of Principles begins with Kant drawing a distinction between these different forms of logic. Here Kant argues that pure general logic cannot give us any way to subsume under rules (which, recall, is precisely the role of the faculty of judgment as described in §2.2.1): 'since general logic abstracts from all content of knowledge, the sole task that remains to it is to give an analytical exposition of the form of knowledge [as expressed (NSK)] in concepts, in judgments, and in inferences, and so to obtain formal rules for all employment of understanding. If it sought to give general instructions how we are to subsume under these rules, that is, to distinguish whether something does or does not come under them, that could only be by means of another rule. This in turn, for the very reason that it is a rule, again demands guidance from judgment.' (A133/B172) This is contrasted with transcendental logic: 'the situation is entirely different for transcendental logic. The latter would seem to have as its peculiar task the correcting and securing of judgment, by means of determinate rules, in the use of the pure understanding…besides the rule (or rather the universal condition of rules), which is given in the pure concept of understanding, it can also specify *a priori* the instance to which the rule is to be applied.' (A135/B174-5) Transcendental logic, then, is concerned with how humans can apply concepts to the objects given in intuition.

> beyond the given concept, viewing as in relation with the concept something altogether different from what was thought in it. This relation is consequently never a relation either of identity or of contradiction; and from the judgment, taken in and by itself, the truth or falsity of the relation can never be discovered. (A154-5/B193-4)

So, the key difference between synthetic and analytic judgments is that in synthetic judgments we need to go beyond the given concept, which requires comparing it with some other concept. But in order to compare a given concept with another 'a third something is necessary, as that wherein alone the synthesis of two concepts can be achieved' (A155/B194). This third thing, Kant says, is the 'one whole in which all our representations are contained, namely inner sense and its a *priori* form, time' (*ibid.*). This demand for our representations to be unified is based upon the findings of the Transcendental Deduction, where Kant argued that our synthetic judgments are possible insofar as they represent concepts as combined in an intuited object.[83]

Kant eventually states the highest principle of all synthetic judgments as follows:

> Every object stands under the necessary conditions of synthetic unity of the manifold of intuition in a possible experience. (A158/B197)

It is precisely these "necessary conditions" that are the synthetic *principles* of pure understanding. Since these synthetic principles are dependent upon the understanding, Kant suggests that they can, quite simply, be read off from the Table of Categories. As with the Table of Categories, the Table of Principles is also divided into the mathematical and dynamical. Corresponding to the mathematical categories of quantity and of quality, Kant derives the axioms of intuition and the anticipations of perception respectively; corresponding to the dynamical categories of relation and of modality there are the analogies of experience and the postulates of empirical thought in general. Of the division of the Table of Principles into mathematical and dynamical, Kant says:

> In the application of pure concepts of understanding to possible experience, the employment of their synthesis is either *mathematical* or *dynamical*; for it is concerned partly with the mere *intuition* of an appearance in general, partly with its *existence*. The *a priori* conditions of intuition are absolutely necessary conditions of any possible experience; those of the existence of the objects of a possible empirical intuition are in themselves only accidental. The principles of mathematical employment will therefore be unconditionally

[83] See (Rosenberg 2005, pp.158-9n) for support for this reading of this passage.

necessary, that is, apodeictic. Those of dynamical employment will also indeed possess the character of *a priori* necessity, but only under the condition of empirical thought in some experience, therefore only mediately and indirectly. Notwithstanding their undoubted certainty through experience, they will not contain that immediate evidence which is peculiar to the former. (A160-1/B199-200)

Having set out the Table of Principles, Kant says a little more about this division:

> It will soon become clear that the principles involved in the *a priori* determination of appearances according to the categories of quantity and of quality…allow of intuitive certainty, alike as regards their *a priori* application to appearances. They are thereby distinguished from those of the other two groups, which are capable only of a merely discursive certainty. This distinction holds even while we recognise that the certainty is in both cases complete. I shall therefore entitle the former principles *mathematical* and the latter *dynamical*. (A161-2/B200-1)

These paragraphs make it quite a lot clearer what is meant by the division of the categories into mathematical and dynamical. The key difference, I take it, is with regard to the mathematical categories' applicability to *pure* intuition specifically. In the Schematism, we saw that Kant urged that geometrical and numerical concepts, like empirical concepts, were schematised as a process: for example the concept of number seems to consist in the process of counting by—something like—keeping a tally of individual marks. These marks can be made in the imagination as well as in the realm of experience: however it is the fact they need not exist that renders them mathematical. The dynamical principles, by contrast, deal with the question of the existence of, and relations between, intuitions.[84]

It is also worth mentioning briefly—since this will be relevant to the discussion of §2.4—that in the context of natural science there is also a distinction between the mathematico-mechanical and the metaphysico-dynamical,[85] which is drawn as follows:

> But now as to the procedure of natural science with respect to the most important of all its tasks—namely that of explaining a potentially infinite *specific variety of matters*—one can take only two paths in this connection: the *mechanical*, by combination of the absolutely full with

[84] Guyer (1987, pp.185-8) characterises the difference between dynamical and mathematical categories as being distinguished by their degree of certainty: the mathematical principles are constitutive because they are certain, whereas the dynamical principles are regulative because they are uncertain, So: 'a principle such as that of universal causation is merely regulative because it is indeterminate: For any given event it tells us that there is some cause or other, but not what the cause is' (p.188). I think that this is not the best way to characterise the sense in which dynamical principles are regulative: I develop an alternative account in §2.3.2.

[85] Here I adopt Warren's terminology (2001, p.63) in order to make clear that there is a relationship between the mechanical concept of matter and the mathematical categories.

> the absolutely empty, and an opposing *dynamical* path, by mere variety in combining the original forces of repulsion and attraction to explain all difference of matters. (2004, pp.71-2; 4:532-3)

Here the distinction, similarly, can be sketched in terms of the extent to which experience is involved. In the mathematico-mechanical case no experience is needed: we rely only on the concepts of the absolutely full and the absolutely empty. The dynamical approach, though, relies on the interaction of bodies—and parts thereof—in stressing the importance of forces.

Now that the basics of Kant's system are in place, we are in a position to address the specific problem with which this chapter is concerned: that is, how do the pure concepts of the understanding and the ideas of reason contribute to establishing universal gravitation as an empirical law?

2.3. Regulative and Constitutive Principles

In Kant's philosophy of science there is a role for both constitutive and regulative principles. The distinction between these principles is usually drawn along the following lines: the constitutive principles are those that govern the function of the understanding and are necessary conditions of experience, whereas regulative principles—the "ideas of reason"—govern the function of reason and are not instantiated in experience in the same way. The matter is not quite this simple though, because regulative principles must still function in order to guide empirical enquiry. Kant is clear about this in a number of places, e.g., in the *Critique of Pure Reason*, he writes:

> We declare…that the things of the world must be viewed *as if* they receive their existence from a highest intelligence. The idea is thus really only a heuristic, not an ostensive concept. It does not show us how an object is constituted, but how, under its guidance, we should *seek* to determine the constitution and connection of the objects of experience. If, then, it can be shown that the three transcendental ideas (the psychological, the cosmological and the theological), although they do not directly relate to, or determine, any object corresponding to them, nonetheless, as rules of the empirical employment of reason, lead us to systematic unity, under the presupposition of such an *object in the idea*; and that they thus contribute to the extension of empirical knowledge without ever being in a position to run counter to it, we may conclude that it is a necessary maxim of reason to proceed in accordance with such ideas. (A670-1/B698-9)

This would seem to suggest that there *is* a role for regulative principles in the construction of experience: they guide the manner in which we should seek to determine the constitution of objects. Friedman (1991, p.73) points out that this distinction between constitutive and regulative principles is also mirrored in the distinction between determinative and reflective judgment. This is because in determinative judgments the universal is given, a priori, by the understanding and particulars are merely subsumed under it. In reflective judgments, on the other hand, the universal is not given a priori: it must be found: to find a universal reflective judgment relies upon a *regulative* transcendental principle which postulates the unity and coherence of empirical concepts and laws so that empirical science can be *systematised*. Kant describes the operation of reflective judgment thus:

> Now the principle sought can only be this: as universal laws of nature have their ground in our understanding, which prescribes them to nature (though only according to the universal concept of it as nature), particular empirical laws must be regarded, in respect of that which is left undetermined in them by these universal laws, according to a unity such as they would have if an understanding (though it be not ours) had supplied them for the benefit of our cognitive faculties, so as to render possible a system of experience according to particular natural laws. (2007, p.16; 5:180)

So, it is in assuming unity and coherence of our system of knowledge that reflective judgment depends upon features of experience that cannot be known a priori: the assumption of unity and coherence is thus a regulative ideal.

The task of this section is to draw out some problems with attempting to assign a sharp distinction between the operation of constitutive and regulative principles in developing a Kantian account of natural science. The root of the problems is that the putative constitutive principles for Newtonian science seem to have a degree of empirical content: as such the clear distinction between the roles of the pure concepts of the understanding and ideas of reason in providing knowledge of the world seems to begin to blur.

2.3.1. *Two problems with the division between constitutive and regulative principles*

Initially it might appear that there should be a clear distinction between constitutive and regulative principles. Constitutive principles are known a priori and are conditions for the very possibility of experience and derive from the understanding; regulative principles do not constitute objects of experience, instead they prescribe the maximal unity and coherence—systematicity—at which our knowledge of nature aims and are within the purview of reason and reflective judgment. However, the matter is not as clear cut as this, which has led to dispute between Buchdahl (1969) and Friedman (1991, 1992a) about the precise role of constitutive and regulative principles in science.

There are two main problems with taking there to be a sharp distinction between constitutive principles as deriving from the understanding and regulative principles as deriving from the faculty of reason in the critical period.[86] The first problem relates to the division, that was discussed in §2.2.2, of the categories into mathematical and dynamical: in the third analogy of experience, Kant claims that this distinction can be regarded as a distinction between constitutive and regulative:

> An analogy of experience is…only a rule according to which a unity of experience may arise from perception. It does not tell us how mere perception or empirical intuition in general itself comes about. It is not a principle *constitutive* of the objects, that is, of appearances, but only *regulative*. The same can be asserted of the postulates of empirical thought in general, which concern the synthesis of mere intuition (that is, of the form of appearance), of perception (that is, of the matter of perception), and of experience (that is, of the relation of these perceptions). They are merely regulative principles and, and are distinguished from the mathematical, which are constitutive, not indeed in certainty—they both have certainty a priori—but in the nature of their evidence, that is, as regards the character of the intuitive (and consequently of the demonstrative) factors peculiar to the later. (A180/B22-3)

As we saw in §2.2.2, the Analogies of Experience are the synthetic principles that correspond to the category of relation. Kant is quite clear that he does not take the Analogies to constitute the actual object of experience; instead they provide a rule for unifying experience in precisely the manner that regulative principles do. So, while the dynamical pure concepts of the understanding can be known a priori, they do not have the same role as the mathematical concepts. Given that constitutive principles are generally taken quite straightforwardly to be those derived from the understanding whereas regulative principles are taken to be at work only in reflective judgment and the faculty of reason, what should we make of some pure concepts of the understanding having a regulative role?

The second problem relates to the constitutive ground of the understanding more generally, and it is chiefly here that Buchdahl and Friedman disagree. Buchdahl (1969,

[86] There is a further difficulty that arises in the post-critical period in *Opus Postumum*, but this impacts only minimally on our present concerns. For details see Friedman (1991, pp.77-8).

pp.499-501) argues that, for Kant, empirical causal laws can only be extracted from an already-given objective sequence of individual events by the inductive procedure that is governed by the reflective power of judgment. Let us examine Buchdahl's analysis of Kant's claim that the possibility of cognition of a sequence of states of an object necessarily presupposes the concept of a cause.

The central passage in question is from §26 of the B-version of the *Transcendental Deduction*. Here Kant discusses the role of the categories in the synthesis of apprehension, which is required for knowledge of objects. He writes:

> [All] possible perceptions, and therefore everything that can come to empirical consciousness, that is, all appearances of nature, must, so far as their connection is concerned, be subject to the categories. Nature, considered merely as nature in general, is dependent upon these categories as the original ground of its conformity to law…Pure understanding is not, however, in a position, through mere categories, to prescribe to appearances any *a priori* laws other than those which are involved in a *nature in general*, that is, in conformity to law of all appearances in space and time. Special laws, as concerning those appearances which are empirically determined, cannot in their specific character be *derived* from the categories, although they are one and all subject to them. To obtain any knowledge whatsoever of these special laws we must resort to experience. (B164-5)

The idea here is that the pure concepts of the understanding provide us with a general idea of nature and provide the most basic conditions for our knowledge of it. However if we wish to know any specific laws, experience is required.

So, in the case of our intuiting a sequence of states of an object, Buchdahl stresses that while in general it is a necessary presupposition of our taking this to represent one object in a variety of states that our intuitions are subsumed under the category of relation, there is also a contingent aspect of the experience:

> It should be stressed with particular emphasis that the sequence whose notion (as an object of cognition) presupposes the concept of cause, and is thus made possible (a possible object of experience) may be, and indeed as such *must* be, regarded as an altogether *contingent* event…in being simply an individual particular happening. (Buchdahl 1969, p.500)

So, the problem here for trying to establish something as an empirical law, is that the happenings that are made possible by the concept of causation are contingent in the sense that they need not have been observed: 'The question whether some observed event or change of state is an instance of an empirical law can be determined only by those inductive procedures distinctive of all scientific enquiry' (*ibid.*). The most that we can say is the following:

> [Since] the notion of 'nature' contains the concept of cause, 'the *understanding* has all it can *demand*' if in the context of scientific enquiry—employing the principle of causality that we 'here require'—we (i.e. 'reason') 'search after and formulate' the 'natural conditions of natural events'. So to explicate the existence of empirical law-likeness is a task for 'reason'; the very definition of the concept of empirical law (as an entity with a necessitarian logic) will have to be given in terms of the activity of reason. (*ibid.*, p.501)

The formulation of empirical laws, for Buchdahl, then, seems to be a task for reason, primarily because the very contingency of the observations of instances of empirical laws means that completely specifying the content of the empirical law is impossible.

Friedman, by contrast, argues that regulative function of the intellect has a lesser role to play in deriving the empirical laws of mathematical natural science. Friedman looks at Newton's law of gravitation and argues that the laws of motion play a constitutive role in deriving the empirical law of gravitation. I will address the success of Friedman's response to Buchdahl in §§2.4-5. In what remains of this section I will focus on dissolving the tension involved in Kant's claim that the dynamical categories have a regulative rather than constitutive function.

2.3.2. Regulative Categories and the Analogies of Experience

In this section I address the first of the two problems outlined above: that is, as to how Kant can claim that the dynamical categories are, in a sense, regulative. I suggest that we can make sense of this by treating the categories as conditions of the possibility of experience in the most general sense while allowing for the empirical manifold—and regulative principles—to play a role in providing specific details. Kant's treatment of this problem goes as follows:

> In the Transcendental Analytic we have distinguished the *dynamical* principles of the understanding, as merely regulative principles of *intuition*, from the *mathematical*, which as regards intuition, are constitutive. None the less these dynamical laws are constitutive in respect of *experience*, since they render the *concepts*, without which there can be no experience, possible *a priori*. But the principles of pure reason can never be constitutive in respect of *empirical concepts*, for since no schema of sensibility corresponding to them can ever be given, they can never have an object *in concreto*. (A664/B692)

The mathematical principles of the understanding are, then, for Kant constitutive of *intuition*; the dynamical principles of the understanding are *regulative* with respect to intuition but constitutive with respect to experience. It is not very clear what this claim amounts to though. Intuition for Kant can be either pure or empirical. In both cases it is the means by which an object is brought before the understanding. The sensibility is affected by empirical intuitions, which relate directly to an object. Pure intuitions are more like the mathematical constructions in thought that we saw in the discussion of the Schematism in §2.2.2. I take it that experience seems to most naturally result from bringing empirical intuitions under the categories.

The dynamical synthetic principles, detailed in the Analogies of Experience, then, are meant to be regulative with respect to intuition and constitutive with respect to experience. It is not immediately clear what is meant in either case, however, the discussion of the division of the Table of Categories into mathematical and dynamical can help us to understand what Kant had in mind. The mathematical categories, recall, pertain as much to pure intuition, e.g. the use of the imagination to construct geometrical figures in thought. That is, the application of these categories enables one to construct intuitions. This is not the case for the dynamical categories which are concerned with only empirical intuitions: that is, the *existence* of an empirical intuition and the *relation* between them. Especially in respect of the concern with the relation between intuitions it seems as if Kant's considerations, at this point in the Critique, are approaching the realm of natural science: it is therefore natural, I think, to expect regulative principles—especially systematicity—to play a role in determining the relations between intuitions.

The other question is as to what it means for the dynamical synthetic principles to constitute experience. In the above passage Kant claims that these principles are constitutive in the sense that "they render the *concepts*, without which there can be no experience possible, *a priori*". What concepts are rendered possible? Friedman (1991, p.179) stresses that here Kant must surely be referring to *empirical* concepts, for Kant immediately draws a contrast between the dynamical laws—which "render the *concepts*…possible"—and the "principles of pure reason", which "can never be constitutive in respect of *empirical concepts*". Friedman points to the following passage as further support of this interpretation:

> But if we consider them in themselves in relation to their origin, these fundamental propositions of pure understanding are anything rather than knowledge based on concepts. For they would not even be possible *a priori*, if we were not supported by pure intuition (in mathematics), or by conditions of a possible experience in general. That everything that

happens has a cause cannot be inferred merely from the concept of happening in general; on the contrary, it is this fundamental proposition which shows how in regard to that which happens we are in a position to obtain in experience any concept whatsoever that is really determinate. (A301/B357)

Kant makes a similar point with respect to determinative judgment in the *First Introduction* to the *Critique of Judgment* (5:212): here Kant suggests that determinative judgments, too, make empirical concepts possible.

The task of this section is to try and make sense of these dual claims: first, that the dynamical principles have a regulative role to play with respect to intuitions and, second, that they are constitutive with respect to empirical concepts. Let us begin by examining the claim that the dynamical principles are regulative with respect to intuition. I suggest that here Kant is best understood as claiming that the dynamical principles—and I will focus on the Analogies of Experience to illustrate this—are regulative with respect to intuition in the sense that temporally distinct appearances must be thought as related so that they can ultimately be grouped together as elements of one temporally extended experience.[87]

Let us look at the Analogies of Experience to clarify this claim. Recall, from §2.2.2, that the Analogies are those synthetic principles that state the conditions under which the categories of relation are applied. There are three Analogies, each of which is devoted to proving a separate principle. The Analogies begin with the statement of a general principle:

> Experience is possible only through the representation of a necessary connection of perceptions.[[88]] (B218)

This expresses the general idea that for it to be possible for humans to experience a single unitary time—with temporally situated and related objects within it—it is necessary to represent perceptions as related according to substance-accident, cause-effect and community.

Now, as has been stressed by Watkins (2005, pp.188-95), the Analogies are primarily concerned with the issue of time determinations: that is, how do we know that, e.g., the keyboard in front of me and the table upon which it rests exist at the same time? Watkins claims that this is a special problem for Kant for two reasons. First, we do not perceive time itself:

[87] Here my understanding of the sense in which the dynamical principles are regulative follows Rosenberg's (2005, chs.8 & 10)

[88] In the A-edition this is stated as 'All appearances are, as regards their existence, subject *a priori* to rules determining their relation to one another in time' (A176-7)

> Since time, however, cannot itself be perceived, the determination of the existence of objects in time can take place only through their relation in time in general. (B219)

So, because we do not perceive time—it is, recall, the form of inner sense—we cannot simply assign objective temporal coordinates to every object that we perceive that tell us each moment that the object exists at.

Second, Kant draws a distinction between subjective time and objective time. This is the purpose of Kant's discussions of the ship and the house (A192/B237): though our perception of an object must always be successive, this does not necessarily tell us anything about the object itself. So, in the case of a house we perceive first the door, then a window, then some walls: though these parts of a house are apprehended successively they nevertheless coexist. In the case of a ship moving along a river there, likewise, is it would seem, an objective time order to its movement; there is also a time-order to our representations of the ship by which it is apprehended. In both cases, once one distinguishes subjective temporal relations between our representations of an object from objective temporal relations, one is left with something of a mystery as to how they connect.

The best way to see how Kant resolves this is to look at one of the Analogies. I will look briefly at the Second Analogy, as this has ramifications later on in Kant's derivation of the law of gravitation.

The principle that Kant seeks to prove in this Analogy is:

> All alterations take place in conformity with the law of the connection of cause and effect.[89] (B232)

There has been some dispute about what this precisely means. The weaker reading of this principle takes it to mean only that all events must have a cause. Friedman (1992b) proposes a strong reading of this principle whereby it is understood as the claim that not only must every event have a cause, but that there must also be causal *laws*. That is, every event has a cause and that once we know the *type* of event A that brings about a *type* of effect B, Friedman claims that Kant intends to claim that 'Events of the same kind as A are necessarily followed by, or result in, events of the same kind as B' (p.163).

[89] In the A-edition this is stated as 'Everything that happens, that is, begins to be, presupposes something upon which it follows according to a rule' (A189).

Now, I think that this is not the best way to understand the Second Analogy in isolation: if we just read the Analogy it simply does not seem as if Kant is trying to argue for causal laws. Friedman does present good evidence from other parts of Kant's work that suggests he may, in places, take there to be causal laws, but my present focus is to try to understand the sense in which the dynamical principles are regulative, and this is best done without additionally complicating the issue by taking the argument of the Analogy to be for causal laws.

For the purposes of this section, then, I regard the second Analogy as arguing only that every effect must have some cause. Watkins provides a helpful and clear summary of the main argument of the Second Analogy, which, I think, accurately captures Kant's intention. Watkins reconstructs this argument as follows:

> P1 Apprehension of objects (the subjective order of perceptions) is always successive.
>
> P2 There is a distinction between the subjective order of perceptions and the successive states of an object such that no immediate inference from the former to the latter is possible.
>
> C1 One cannot immediately infer objective succession from the successive order of perceptions.
>
> P3 To have knowledge of objective succession, the object's states must be subject to a rule that determines them as successive.
>
> P4 Any rule that determines objective succession must include a relation of condition to conditioned, i.e., that of causal dependence of successive states on a cause.
>
> C2 To have knowledge of the successive states of an object, the object's successive states must be dependent on a cause, that is, must stand under a causal rule (from P3, P4, and C1). (2005, pp.209-10)

P1, P2 and C1 are intended to correspond to the problem of time-determination. The best bit of textual evidence supporting interpreting the argument in this fashion is as follows:

> [We] must derive the *subjective succession* of apprehension from the *objective succession* of appearances. Otherwise the order of apprehension is entirely undetermined, and does not distinguish one appearance from another. Since the subjective succession by itself is altogether arbitrary, it does not prove anything as to the manner in which the manifold is connected in the object. The objective succession will therefore consist in that order of the manifold of appearance according to which, *in conformity with a rule*, the apprehension of that which happens follows upon the apprehension of that which precedes. Thus only can I be

justified in asserting, not merely of my apprehension, but of appearance itself. (A193/B238)

Some care is required here. Guyer (1987, p.247) stresses that what Kant is concerned to show is that the irreversibility of a sequence of representations is a consequence of the occurrence of a perceived event.[90] P1, I take it, is something that we saw Kant clearly subscribe to in the description of the problem of time-determination. In the above passage we see Kant clearly argue for a form of P2: the subjective succession of representations can be entirely arbitrary and as such one cannot derive anything about objective succession from it, which is C1.

P3 is intended to represent Kant's thinking in the last two sentences of the above quote: he argues here that for objective succession one must order the manifold of appearance in such a way that the first thing that happens is followed by the second in accordance with a rule. P4 then simply characterises this rule as causal. Watkins takes Kant to express this point in the following passage:

> In conformity with such a rule there must lie in that which precedes an event the condition of a rule according to which this event invariably and necessarily follows. I cannot reverse this order…Therefore, since there certainly is something that follows [i.e. that is *apprehended* as following (NKS)], I must refer it necessarily to something else which precedes it and upon which it follows in conformity with a rule that is of necessity. The event, as the conditioned, thus affords reliable evidence of some condition and this condition is what determines the event. (A193-4/B238-9)

The claim that the second event is conditioned, and it is this that provides evidence of some condition that determines the second event, seems a clear characterisation of the rule as causal.[91] These, premises, with C1, then licence the principle that Kant set out to prove.

The above, then, serves as a template of the argument of the Second Analogy. In what sense is this Analogy, as a dynamical synthetic principle, regulative of intuition? The Analogies are regulative with respect to intuition in the sense that they establish the *unity of time* (i.e. the claim that there is only one time, moments of time are successive parts of this one time and that all objects are located in this time). In the introduction to the Analogies, Kant refers to three modes of time: persistence, succession and simultaneity

[90] That is in contrast to the claim that one can derive the occurrence of the perceived event from the irreversibility of the representation.

[91] Here it is worth stressing that this passage seems to provide evidence against Friedman's stronger understanding of the claim of the Second Analogy. It seems clear that at this stage of the argument that Kant can only intend his argument to apply to token events.

(corresponding to the relational categories). Each Analogy of Experience demonstrates the objective validity of these modes of time, so in the Second Analogy we have objective knowledge of succession through the causal relation.

Watkins explains how these three modes of time make the unity of time possible in the following fashion (2005, pp.192-3). Persistence is required because it expresses the idea that time stays the same even as its moments change. Succession and simultaneity are the two basic temporal relationships that all objects must bear to one another: these contribute to the unity of time because for objects to be situated in one and the same time, all objects must be either simultaneous with, succeed or be succeeded by all other objects.

The Analogies of Experience are important for connecting these temporal modes with the objective world, and as such unify "nature". Kant explains this in the introduction to the Analogies:

> By nature, in the empirical sense, we understand the connection of appearances as regards their existence according to necessary rule, that is, according to laws. There are certain laws which first make a nature possible, and these laws are *a priori*…Our analogies therefore really portray the unity of nature in the connection of all appearances under certain exponents which express nothing save the relation of time…to the unity of apperception…Taken together the analogies thus declare that all appearances lie, and must lie, in *one* nature, because without this *a priori* unity no unity of experience, and therefore no determination of objects in it would be possible. (A216/B263)

This is the crucial passage, which, I think, explains the regulative role of the Analogies. There is a strong connection between the unity of time and the unity of nature: temporally distinct appearances must be thought as related so that they can ultimately be grouped together as elements of one temporally extended experience. The role of the Analogies is just to link the unity of time to the unity of experience. However this is only possible by the manner in which the three modes of time contribute to the unity of time. Unifying the modes of time is a regulative procedure (since unification is a regulative ideal), and it is thus a regulative procedure that makes intuitions—situated within a unified experience—possible. It is in this sense, I suggest, that the Analogies of Experience are regulative.

It should now be clear in what sense the Analogies of Experience are regulative with respect to intuition. It remains to ask how Kant understands dynamical synthetic principles as constitutive of experience. That is how do they make empirical concepts possible?

I touched upon empirical concepts in §2.2.[92] It is clear that empirical concepts are, in some sense, derived from experience. For example, the empirical concept of a dog is presumably formed by repeated observation of objects-in-experience that share certain essential canine properties. The immediate concern is that the procedure involved in constructing a universal—i.e. a concept—from particulars is precisely the task that is assigned to reflective judgment in the Critique of Judgment. This, as has been stressed, is a characteristically regulative use of reason. It is, as such, not immediately clear why Kant takes the dynamical principles to have a constitutive role to play with respect to experience.

The issue is clarified somewhat if we consider the most general empirical concept: matter. In the *Metaphysical Foundations of Natural Science* Kant explicitly sets out to examine the role of the understanding and determinative judgment in elucidating the concept of matter. In this work Kant aims to show how mathematics can be applied to pure natural science by providing an account of how the concepts and principles of the understanding are to be brought into a determinate relation with the pure intuitions of space and time:

> But in order to make possible the application of mathematics to the doctrine of a body, which only through this can become natural science, principles for the *construction* of the concepts that belong to the possibility of matter in general must be introduced first. Therefore, a complete analysis of the concept of matter in general will have to be taken as the basis, and this is a task for pure philosophy—which, for this purpose, makes use of no particular experiences, but only that which it finds in the isolated (although intrinsically empirical) concept itself, in relation to the pure intuitions in space and time, and in accordance with the laws that already essentially attach to the concept of nature in general, and is therefore a genuine *metaphysics of corporeal nature*. (2004, p.8; 4:472)

So, in order to provide the "metaphysics of corporeal nature" Kant thinks that an analysis of the empirical concept of matter in general must be provided. This process involves asking as to the properties of matter that make it a priori suitable for application to outer experience (*ibid.*), which is achieved by considering matter as determined by each of the four classes of the table of categories:

> The concept of matter had therefore to be carried through all four of the indicated functions of the concepts of the understanding (in four chapters), where in each a new determination of this concept was added. (2004, p.12; 4:476)

[92] See footnote 79 and the paragraph to which it is attached.

In §2.3 I look in detail at the second chapter of the *Metaphysical Foundations*, in which the empirical concept of matter is articulated according to the categories of quality. For the time being, though, let us keep our focus on the question of how we should understand principles that are constitutive of experience.

Friedman (1991, p.82) suggests that the procedure for articulating the empirical concept of matter follows precisely the methodology suggested for the empirical concept of alteration in the *Second Analogy*, which is described as follows:

> How anything can be altered, and how it should be possible that upon one state in a given moment an opposite state may follow in the next moment—of this we have not, *a priori,* the least conception. For that we require knowledge of actual forces which can only be given empirically, as, for instance, of the moving forces, or what amounts to the same thing, of certain successive appearances, as motions, which indicate [the presence of (NKS)] such forces. But apart from all question of what the content of the alteration , that is, what the state which is altered may be, the form of every alteration, the condition under which, as a coming to be of another state, it can alone take place, and so the succession of the states themselves…can still be considered *a priori* according to the law of causality and the conditions of time. (A206-7/B252-3)

Here Kant is quite clear that the transcendental principle of causality, as a pure concept of the understanding, involves only the most general idea of an alteration. So, the Second Analogy shows that we must take the objects of experience to stand in a causal relationship. This is a necessary feature of our experience of the world and in that sense the dynamical principles are constitutive. However, all that is necessitated by the understanding is that we perceive objects as standing in this causal relation: how precisely this causal relation is empirically understood is left to be resolved. However the fact that there is room for an empirical concept of causation is made possible by the structure of our understanding.

This, I think, is enough to solve the first of the two problems mentioned in §2.2.1. The worry here, recall, is that Kant claims that the dynamical synthetic principles are regulative. In this section I have explained the sense in which this is intended: they are regulative with respect to intuition in the sense that temporally distinct appearances must be unified by the Analogies in order to provide us with a single intuition of the world. This function of the principles is regulative precisely because unification is a regulative ideal. There remains a constitutive role for the dynamical synthetic principles in that they reveal the structure of the understanding which grounds the most general features of our perception of the empirical world. How the empirical concepts corresponding to the synthetic principles are developed remains to be explained.

It is at this point that the second problem mentioned in §2.2.1 reappears. Our concern here is with the extent to which the principles of natural science are constitutive. As we saw in the previous chapter, Friedman's central claim is that the laws of motion play a constitutive role in Kant's reconstruction of Newton's derivation of the law of gravitation. The three laws of motion, for Kant, correspond to the Analogies of Experience: in salvaging a sense in which the Analogies—though they are described by Kant as regulative with respect to intuition—are constitutive of empirical concepts, we have overcome the first threat to the constitutivity of these laws (for how can they be constitutive of Newtonian physics if the synthetic principles from which they are derived are merely regulative?).

Buchdahl's argument, recall, is that empirical laws, such as the law of gravitation, are derived primarily by reason. Friedman urges, instead, that the laws of motion play a constitutive role in the derivation of the empirical law. Before this problem can be addressed directly there is a subsidiary worry: on the account provided so far the laws of motion are empirical concepts that are made possible by the structure of our understanding. However, the content of these empirical concepts is known only through experience. It seems that here there is a further place in which reason in its regulative employment may play a role in the development of empirical laws. In particular the very basis of the empirical law seems to be based on the empirical claim that matter has a fundamental attractive force. Kant's account of how matter can be known to possess an intrinsic attractive force is worth examining in some detail to see just how large a role experience and regulative reasoning have to play in this key empirical concept.

2.4. The role of determinative judgments in attributing attractive force to matter

In this section then, I investigate the grounds upon which Kant takes matter to be imbued with attractive force. In the following section I move on to consider the role of the laws of motion in the construction of an empirical law that describes the action of this attractive force. In the previous section I have attempted to explain how it is that Kant's constitutive procedure stretches further into the empirical realm than one would initially assume: in agreement with both Friedman and Buchdahl, I have suggested that the dynamical synthetic principles are constitutive of experience in the sense that they provide a general framework for knowledge to which empirical detail must be added.

How is it that the content of the empirical concept is known in the first place? That is, while there is a straightforward sense in which, e.g., matter is the empirical correlate for substance, how is it that we come to know anything about the nature of matter? The temptation is to assume that the content of the empirical concept is discovered through the reflective use of judgment: this, after all, is the process by which a concept is derived from its individual instantiations. Given that the reflective power of judgment is governed by regulative principles, this seems to cast doubt on the idea that the principles of pure natural science are constitutive rather than regulative.

However, it is relatively clear that Kant takes the constitutive principles of determinative judgment to play a key role in determining the content of empirical concepts. This is explained in the *Critique of Judgment* in the following fashion:

> A transcendental principle is one through which we represent *a priori* the universal condition under which alone things can become objects of our cognition generally. A principle, on the other hand, is called metaphysical, where it represents *a priori* the condition under which alone objects whose concept has to be given empirically, may become further determined *a priori*. Thus the principle of the cognition of bodies as substances, and as changeable substances, is transcendental where the statement is that their change must have an *external* cause. For in the first case bodies need only be thought through ontological predicates (pure concepts of the understanding), e.g., as substance, to enable the proposition to be cognised *a priori*; whereas in the second case, the empirical concept of a body (as a movable thing in space) must be introduced to support the proposition, although, once this is done it may seem quite *a priori* that the latter predicate (movement only by means of an external cause) applies to bodies. (2007, pp.16-7; 5:181)

In this section I aim to explain why it is that Kant takes the determination of the content of an empirical concept to be an a priori process: I will do this by means of Kant's discussion of matter considered in relation to the categories of quality. This is a particularly helpful part of Kant's discussion to look at because it is here that Kant argues that attractive force is a fundamental property of matter, which has a central role to play in Kant's discussion of the status of the universal law of gravitation (which is the topic of §2.4).

In the "Metaphysical Foundations of Dynamics", in which Kant brings matter under the categories of quality, Kant considers matter as "*movable* insofar as it *fills a space*":

> Matter fills a space, not through its mere *existence*, but through a *particular moving force*. (2004, p.34; 4:497)

This latches onto then-contemporary debate about how precisely matter fills space. There were two schools of thought on this subject: Kant refers to these as "mathematical-mechanical" and "metaphysical-dynamical".

The distinction between these explanations of how matter fills space is in line with the distinction between the mathematical and the dynamical sketched in §2.1. The mathematical-mechanical approach is attributed to Lambert (2004, pp.34-5; 4:497-8) and is characterised by the claim that matter fills space through possessing the property of *solidity*. This is a somewhat mysterious property of matter, that Warren (2001, p.64) has convincingly argued is understood by Kant to amount to the claim that matter is incompressible. Kant expresses this view in *Explication 4*:

> The *filling of a space* with absolute impenetrability can be called *mathematical* filling of space whereas that with mere relative impenetrability can be called *dynamical* filling of space…According to the purely mathematical concept of impenetrability (which proposes no moving force as originally belonging to matter), matter is not capable of compression except insofar as it contains empty spaces within itself. (2004, pp.38-9; 4:502)

So, on the mathematical-mechanical account, matter cannot be compressed. This is because, on this account of matter, space is either filled or it is not: it is not a matter of degree. As such if matter gets smaller it is not because it is being compressed, rather it is because there is less available space for it to fill.

This is contrasted with the metaphysical-dynamical account which posits only relative impenetrability:

> [According] to our discussion of this property, impenetrability rests on a physical basis. For expanding force first makes matter itself possible, as an extended thing filling its space. But this force has a degree that can be overpowered, and thus the space of its extension can be diminished, that is, penetrated up to a certain amount by a given compressing force, but only in such a way that complete penetration is impossible, because this would require an infinite compressing force; *therefore the filling of space must be viewed only as relative impenetrability*. (2004, p.39; 4:502)

On the metaphysical-dynamical account, then, because matter is taken to fill space by a fundamental repulsive force, matter can be compressed. This is simply because the repulsive force has a magnitude, if a greater force is applied to matter then the repulsive force is diminished and matter fills less space.

As Kant's referring to this view of matter as *mathematical*-mechanical suggests, Kant understands this explanation of matter's filling space as an attempt to construct an a priori, geometrical physics. This is very clear in his discussion of Lambert's physics. When two bodies collide they do not pass through each other, there is some resistance. Where, on the mechanical account, does this resistance originate? Kant answers:

> [The] presence of something *real* in space must already, through its concept, and thus in accordance with the principle of noncontradiction, imply this resistance and bring it about that nothing else can be simultaneously in the space where such a thing is present. But the principle of noncontradiction does not repel a matter advancing to penetrate into a space where another is found. (2004, pp.34-5; 4:497-8)

This approach, then, is taken to be an attempt to derive physics from logical principles like that of noncontradiction. An object fills its space through its solidity, only one object can occupy any space at a time and it is a contradiction for two bodies to exist in the same space at any given time. Kant argues, though, that more than this is needed for a contradiction: "Only when I ascribe to that which occupies a space a force to repel every external movable that approaches, do I understand how it contains a contradiction for yet another thing of the same kind to penetrate into space occupied by a thing" (2004, p.35; 4:498).

Having dismissed this account of how matter fills space, Kant advocates the dynamical approach: "Matter can be *compressed* to infinity, but can *never* be *penetrated* by a matter, no matter how great the compressing force of the latter may be." (2004, p.37; 4:501). Kant's argument for the metaphysico-dynamical view of matter is as follows:

> Penetration into a space (in the initial moment this is called a striving to penetrate) is a motion. Resistance to motion is the cause of its diminution, or even the change of this motion into rest. Now nothing can be combined with a motion, which diminishes it or destroys it, except another motion of precisely the same moveable in the opposite direction (Phoron. Prop.). Therefore, the resistance that a matter offers in the space that it fills to every penetration by other matters is a cause of the motion of the latter in the opposite direction. But the cause of motion is called a moving force. Thus matter fills its space through a moving force, and not through its mere existence. (2004, p.34; 4:497)

So, consider any piece of matter that you have to hand and think about exerting force on it. Whether you exert this force by rolling objects at your piece of matter or just squeezing it between your hands you meet with resistance. This resistance is the cause of the change in motion in either the objects you were rolling or your hands.

At this point, Kant appeals to the Remark to the Explication 5 from the Phoronomy to explain how motions are diminished. This Proposition reads "The composition of two motions of one and the same point can only be thought in such a way that one of them is represented in absolute space, and instead of the other, a motion of the relative space with the same speed occurring in the opposite direction is represented as the same as the latter" (2004, p.26; 4:490). Any given motion can only be cancelled out by an equal motion in the opposite direction. So a change in the motion of an object upon its striking of another object can only be explained by the presence of another motion originating in the second object. Motion is caused by moving force: so, matter must fill its space through a moving force.

But, why can't mechanists say that their incompressible bits of matter exert a moving force when something collides into them? In trying to answer this we need to think of how Kant understands the alternative to his view: how does Kant understand the claim that matter fills space through its "mere existence"? Warren (2001, p.70) provides a plausible interpretation of Kant's thought here. Kant, he points out, uses the phrase "mere existence" in a similar context in two of his earlier work: his *Inaugural Dissertation* (1992 [1770]) and *Nova Dilucidatio* (1755). In these works Kant argued that the causal interactions of distant objects cannot be explained by the mere existence of the relevant objects; as in the Metaphysical Foundations, Kant considered the manner in which bodies (or pieces of matter) causally interact. Furthermore—and very suggestively—Kant seems to take the phrase to be interchangeable with "mere subsistence". This would seem to be the case in this passage from the *Inaugural Dissertation*:

> Given a plurality of substances, a principle of possible mutual interaction is not given by their mere existence; something more is required from which their mutual relations may be understood. Through their mere subsistence they do not necessarily refer to anything else, except perhaps to their cause. (1992, p.401; 2:407)

The equation of mere existence and mere subsistence, seems to remain present in the Metaphysical Foundations: he describes the view of how matter fills space to which he intends to object as the claim that "solidity must be assumed in everything that exists (substance)". Referring to "everything that exists" as "substance" in this fashion likewise suggests that Kant understands the mechanists to promote a view of matter as a substance.

Now, a substance was taken to be something that could be understood apart from its relations to all other things, that is, in virtue of its own inner determinations. This leads Warren to suggest that we ought to understand the first proposition as meaning this:

> Matter fills a space, not [in virtue of how it is apart from all relations to others, i.e. in virtue of its inner determinations], but by a special moving force. (Warren, 2001, p.73)

With the proposition understood in this way, we are now in a position to see why Kant thinks that the mechanist approach cannot account for collisions between matter.

Maybe it is worth considering how an argument to impenetrability from the principle of non-contradiction alone might go. Consider a Lockean type argument, whereby it is claimed that if two objects fill the same space then, in fact, it is just one object. The problem here is that this type of explanation does not provide any causal grounds for explaining the impenetrability of matter. This, really, is the nub of the matter: understanding the universe is about understanding the interaction of matter. This is simply not possible if we consider the properties of an individual piece of matter by its inner determinations only and in isolation from all other pieces of matter.

Kant's aim is to give an account of the reciprocal interactions of pieces of matter. Matter in itself, devoid of any relation is not a possible object of mathematical construction (see for instance Galilean relativity) as well as a possible object of experience (because of the transcendental idealism). Plus the metaphysical assumption of all Kant's system is that the actual world is constituted by actual mutual interactions of which we can determine the form, both a priori and a posteriori. In this context one has to ascribe his view of the interplay of attraction and repulsion.

Having established how matter fills space, Kant is in a position to consider the makeup of bodies: in particular he is concerned with the relationship between different *parts* of material bodies. In proposition 2 Kant shows that each part of some piece of matter repels the other parts: through mutual repulsion of its parts every piece of matter has a tendency towards expansion. Furthermore, Kant is clear that the repulsive force does not have a limit to the distance over which it is effective: "because smaller degrees are possible to infinity for any moving force" (2004, p.46; 4:508). Kant is able to conclude:

> Hence, matter, by its repulsive force…would, alone and if no other moving force counteracted it, be confined within no limit of extension; that is it would disperse itself to infinity, and no specified quantity of matter would be found in any specified space. Therefore, with merely repulsive forces all spaces would be empty, and thus, properly speaking, no matter would exist at all. (*ibid.*)

How is this problem to be avoided? Quite simply, Kant postulates another, attractive, force of matter that operates in an opposite direction to the repulsive force.

The second part of the argument is the inverse of this: Kant argues from the attractive force of matter to the repulsive force. The argument goes as follows:

> Active force is that moving force of matter whereby it impels another to approach it; consequently…nothing can hinder the action of a moving force except another moving force opposed to it, and that which opposes attraction is repulsive force. Hence, without repulsive forces, through mere convergence, all parts of matter would approach one another unhindered, and would diminish the space they occupy. (2004, p.48; 4:510-1)

In short, if there were only attractive forces, then all matter would be coalesced into a "mathematical point" (2004, p.49; 4:511). Since this is not the case, we can conclude that both attractive and repulsive forces must be at work.

Kant's arguments, I suggest, are best understood as modus tollens arguments, for example the repulsion-entails-attraction argument can be reconstructed as follows:

(i) *If P then Q*: if matter fills space only through repulsive force then "no specified quantity of matter would be found in any specified space".
(ii) $\neg Q$: but specified quantities of matter *are* found.
(iii) *Therefore* $\neg P$: therefore matter cannot fill space through repulsive force alone.

Viewing the balancing arguments as having this structure explains why Kant includes both repulsion-entails-attraction and attraction-entails-repulsion forms of the argument when one might expect just the repulsion-entails-attraction argument to be sufficient for Kant's purpose: it allows him to conclude that in either the case that matter were equipped solely with attractive force or the case that matter were equipped solely with repulsive force we would be in conflict with experience. This being so we are forced to conclude that matter must be equipped (given the argument of proposition 1) with both forces. While these arguments have initial appeal, closer reflection reveals both arguments to be quite problematic.[93]

(i) Repulsion-entails-attraction: A straightforward objection here is that it simply does not follow from the idea that matter is possessed only of a repulsive force that all matter would disperse to infinity and that, as a consequence all spaces would be empty. This is because the application of a finite force (that diminishes over distance) will never move matter to an infinite distance: there is no limit to how far it will propel matter, but it will always only be a finite distance.

[93] See (Warren, 2010, pp.200-2).

(ii) Attraction-entails-repulsion: The problem here is that it is not obvious why all matter must coalesce to a point. This can be seen quite clearly if we imagine an object exerting a strong attractive force on an object exerting a much weaker attractive force. The more attractive object will, roughly, stay still. Now, the less attractive matter's behaviour is, properly, determined by its initial velocity. If it's stationary it will be drawn towards the more attractive matter. *But* there is no reason for it not to pass straight through the more attractive matter: recall, what renders matter impenetrable is its repulsive force, and, *ex hypothesi*, this has been removed. So here our less attractive matter would simply oscillate through the more attractive matter. Alternatively, if its initial velocity was appropriately valued and directed perpendicular to the direction towards the more attractive matter, then it could even orbit it.

Both counterarguments work by denying the first premise of Kant's arguments. Warren has suggested that the interpretative problem here derives from attributing inertial properties to matter earlier than is warranted within Kant's system: i.e. instead of treating force as a change of acceleration, Warren suggests that at this stage in the *Metaphysical Foundations*, Kant treats a force as only a change in configuration.[94] Kant's definitions of attractive and repulsive force seem to bear out a reading of force in the Dynamics as only being understood as a change in configuration.

> Attractive force is that moving force whereby a matter can be the cause of the approach of matter to itself (or, equivalently, whereby it resists the withdrawal of other matter from itself). Repulsive force is that whereby a matter can be the cause of making other matters withdraw from itself (or equivalently whereby it resists the approach of other matter to itself). (2004, p.35; 4:498)

> Beyond every expanding force a greater moving force can be found. But the latter can also act contrary to the former, whereby it would then decrease the space that the former strives to enlarge…Therefore, for every matter a compressing force must also be discoverable, which can drive it from the space it fills into a decreased space. (2004, p.37; 4:500)

What is immediately striking is that in this discussion of force there is no mention of mass (or quantity of matter, as Newton treated it) in either paragraph. Nor have I found

[94] Warren offers a more detailed argument for this than space permits me to get into, see his (2010, pp.217-28) for a full discussion. In particular he emphasises, as I do not, that remnants of the *vis viva* debate can be found in the chapter on Dynamics.

reference to either term in the chapter on Dynamics: the first mention of "quantity of matter", is in the Mechanics and appears in Kant's definition of quantity of motion (that is, momentum):

> The quantity of motion of bodies is in compound ratio to that of the quantity of their matter and their speed. (2004, p.77; 4:538)

So, I—with Warren—suggest, that the understanding of force in the dynamics is one in which matter is taken to have zero quantity of motion and as lacking inertial properties. Hence throughout the dynamics, as we have seen, forces are described as affecting motion only.

On this reading of the chapter on *Dynamics*, the modus tollens arguments are more defensible. Recall that on the attraction-entails-repulsion branch of the argument our concern was that it was not clear that matter would compress down to a single point: might we not have oscillating systems? If there were no inertial properties to matter, oscillating systems would not be possible. The matter is somewhat less clear on the repulsion-entails-attraction branch. Here the concern was that matter would not be infinitely extended if there was no attractive force, because applying a finite force for a limited period of time does not lead to infinite separation. It is, at least, more plausible that matter would be infinitely extended by constant application of repulsive force if matter lacked inertial properties.

Now, this is of importance with respect to the current project because it affects the extent to which we consider the dynamical conception of how matter fills space as empirical. Friedman (1991, p.84) stresses the empirical nature of the metaphysical-dynamical approach, claiming that it is beyond reason to comprehend original forces according to their possibility. He cites the following passage as evidence:

> If the material itself is transformed into fundamental forces (whose laws we cannot determine a priori, and are even less capable of enumerating reliably a manifold of such forces sufficient for explaining the specific variety of matter), we lack all means for *constructing* this concept of matter and presenting what we thought universally as possible in intuition (2004, p.63; 4:525)

If we interpret Kant in this way, then, the role of experience in shaping Kant's judgment as to the manner in which matter fills space is quite minimal. The only empirical knowledge that Kant needs is that matter resists penetration (and that the universe is neither infinitely expanded nor a mathematical point). This is enough to reject the mechanist approach and derive that attractive force must also be an essential property of matter.

This I suggest is an exemplar of what might be meant by a principle that is constitutive of experience. The category of quality provides the most general conditions for the possibility of experiencing substance; the experience of matter as resisting penetration is then sufficient to allow this purely constitutive principle to be transformed into a principle of natural science, which remains constitutive but not entirely divorced from experience.

2.5. Establishing the universal law of gravitation

In the previous section we saw that, for Kant, attractive and repulsive forces are derived a priori as belonging to the empirical concept of matter. This, I take it, resolves any concern about Kant being committed to the inductive procedure of reflective judgment as a means for developing the principles of pure natural science. However even if attractive force is a fundamental property of matter, this does not amount to the empirical law that governs the interaction of material bodies. More is needed to get to the universal law of gravitation. This leaves us to answer Buchdahl's concern—expressed in §2.3.1—that it is regulative principles that play the chief role in determining specific empirical laws. In this section I will look at Kant's derivation of the Newton's law of gravitation and argue, with Friedman, that this is best understood by assigning a constitutive role to the laws of motion. However, the matter is not entirely straightforward, and there seems to be a key role played by regulative reasoning in deriving the laws of motion which are intended to be constitutive with respect to Newton's theory. In this section I aim to clarify the role of both reason and the understanding in Kant's account of the universal law of gravitation, and I will suggest that this need not diminish the role of the understanding in the manner that we saw Buchdahl suggest it might in §2.3.1.

To begin with I will suggest a crude starting point for interpreting Kant's argument about the derivation of the law of universal gravitation; I will refine this account in the rest of the section. So, for Kant the understanding provides the basis for all knowledge, and is guided by the pure concepts that are given in the table of categories. Of particular importance in Newtonian physics are those concepts corresponding to relation: *of inherence and subsistence, of causality and dependence* and *of community* (A80/B106). In the third chapter of the *Metaphysical Foundations*, these become:

(1) In all changes of corporeal nature the total quantity of matter remains the same, neither increased nor diminished. (2004, p.80; 4:541)

(2) Every change in matter has an external cause. (Every body persists in its state of rest or motion, in the same direction, with the same speed, if it is not compelled by an external cause to leave this state.) (2004, p.82; 4:543)

(3) In all communication of motion, action and reaction are always equal to one another (2004, p.844:545).

Kant attempts to derive each of these principles from the corresponding Analogy of Experience.

These are Kant's equivalents of Newton's laws: (1) the principle of the conservation of mass, (2) the law of inertia and (3) a principle of action and reaction. Friedman (1992a, p.168n) notes that, while these generally seem easily convertible to Newton's equivalent laws, we may have concern over the form of Kant's third law, which—on Friedman's reading—implies Newton's second law, which is normally expressed as F=ma.[95] Kant, though, formulates his principle in terms of momenta. This means that in modern terms and for two bodies, e.g. *A* and *B*, Kant's principle is best expressed as stating that $\mathrm{m}_{\mathrm{A}} a_A = -m_B a_B$. Because the third law remains about the equality of action and reaction it is clear that it relates to forces. So this previous equation entails that $F_{AB} = - F_{BA}$, which suggests that Kant's third law presupposes Newton's second law.

This contributes towards Friedman claiming that Kant takes Newton's derivation of the law of gravitation, given in Book III of the *Principia*, as his model for deriving his own law of gravitation.[96] So, before we look at Kant's derivation of the law of gravitation it will be helpful to provide a brief summary of Newton's argument for the inverse-square law.

[95] There is a significant debate in the literature both about the extent to which Kant's version of the laws of motion corresponds to Newton and about whether Kant even *intended* his laws to correspond to Newton's. At first glance there are at least three serious problems in taking Kant's laws to correspond to Newton's. First, Kant states the conservation of quantity of matter as a principle whereas Newton is silent on whether quantity of matter must be conserved; second, Kant does not mention Newton's second law at all; third, Kant's law of inertia is stated in terms of "change of matter" and "external cause" rather than motion and forces. See Watkins (1997) for a detailed discussion of the differences between Kant and Newton's statements of the laws of motion. For Watkins, Kant should be read as being influenced by a wider variety of sources than just Newton: in particular his laws of motion should be understood as being influenced *both* by the Newtonianism of members of the Berlin Academy of Sciences— *and* by Kant's pre-Critical Leibnizianism.. Friedman now accepts that Kant's laws of motion *do* have a "Leibnizian purview" (2013, p.282n35). He argues, however, that this in itself does not mean that Kant did not seek to provide a metaphysical foundation for Newtonian mathematical physics. This is because, while Kant's laws of motion do have Leibnizian provenance, Kant's critical proofs of the laws entail a radical break from the Leibnizian tradition (Friedman, 2013, p.369n136). For this reason, I accept Friedman's claim that Kant's laws of motion do—albeit it non-straightforwardly—correspond to Newton's laws of motion.

[96] Friedman makes a quite detailed argument for this in chapter 3 of his (1992a). For the most part the argument is quite convincing. The main question mark over his account is over how Newtonian Kant considered the laws of motion to be (see previous footnote). This issue will have some impact on this section, and I will look at Kant's derivation of his second law later on, but I don't think this impacts significantly on Friedman's argument that Kant is modelling his derivation of the law of gravitation of Newton's.

Newton, in the *Principia*, derived the inverse square law from Kepler's observation that the orbits of the planets are elliptical and from Book 1 Proposition XI, which tells us how to work out the centripetal force on a body moving so as to describe an ellipse. In taking Kepler's laws as his starting point, Newton is starting from the phenomena. Proposition XI is a proof—relying on geometry[97] and Newton's laws—that the force towards on a body towards the centre of an ellipse is proportional to the inverse square of the distance from the body to the focus of the ellipse. Newton can then straightforwardly conclude from this that the force which acts upon each planet must vary inversely with the square of the distance from the centre of the Sun.

Friedman (1992a, p.172) points out that this is not sufficient for the full law of gravitation, however, as that states that *any* two bodies in the solar system mutually attract each other with the same inverse square force. Furthermore the force is proportional to the masses of the two bodies in question. The mutuality of attractions is given by Newton's third law; what is less clear is how Newton can derive the *universality* of gravitation so that the inverse square law can be said to apply to any two bodies. Newton argues for universality as follows:

> Universally, all bodies about the earth gravitate towards the earth; and the weights of all, at equal distances from the earth's centre, are as quantities of matter which they severally contain. This is the quality of all bodies within reach of experiments; and therefore (by Rule III) to be affirmed of all bodies whatsoever. (1995, p.332)

Rule III is detailed earlier in the *Principia*, and is just the claim that if we find a particular quality belongs to all the bodies within range of our instruments then we should assume that it is a universal quality of all bodies (p.320).

So the structure of the derivation is as follows. Newton begins with the phenomena of observable relative motions of satellites with respect to primary bodies, taken from Kepler. He then applies the laws of motion to the observable data to get first the inverse square law and then the universality of gravitation.

Let us look at how Kant derives the laws of motion: I will focus on the second law of mechanics, which is Kant's version of the principle of inertia. The proof of this principle proceeds as follows:

> (From general metaphysics we take as basis the proposition that every change has a *cause* and here it is only to be proved of matter that its change must always have an *external cause*) Matter, as mere object of the outer sense has no other determinations except those of

[97] The proof is geometrical in that it proceeds by appeal to "similar triangles" and the "properties of the ellipse" (Newton, 1995, p. 52)

external relations in space, and therefore undergoes no change except by motion. With respect to the latter, as change of one motion into another, or of a motion into rest, or conversely, a cause must be found (by the principle of metaphysics). But this cause cannot be internal for matter has no essentially internal determinations or grounds of determination. Hence every change in a matter is based on external causes. (2004, p.83; 4:543)

This law of mechanics is based upon the findings of the second Analogy of Experience, which is the claim that every change has a cause. The claim that matter has no other determinations except those of external relations in space is a result from the *Phoronomy* (2004, p.17; 4:482). However in the next sentence, Kant shifts the terms of the discussion to *changes of motion*. That is, the argument has shifted from a claim about how objects change—by motion—to a draw conclusions about *changes of motion*.

Buchdahl (1969, p.677) suggests that here Kant is making a tacit assumption: he has to identify 'change of motion' with 'change of velocity' and by 'velocity' he has to understand 'uniform velocity'. For without these assumptions, it is neither clear why velocity itself shouldn't be regarded as a change nor why when there is no change in velocity it should be considered that there is no change of state.

This poses some difficulty for us in interpreting Kant's intention with his derivations of the laws of motion:[98] how deductive are they intended to be? Is Kant, with hindsight, supplying the interpretation of change of motion as change of velocity, as he needs, or is he, instead, to be understood as intending this as a logically sound proof. If the former then we could treat Kant as aiming only to show that the metaphysical foundations are only demonstrations of possibility (in the sense that they display how much in the laws mirrors the general concepts and principles of a science). If this is so, Buchdahl claims:

Such considerations could not be intended as inductive support for the laws, but solely as architectonic devices—not so very different from the procedures we noticed operate at the level of reason. (1969, p.678)

It is difficult to say precisely which of these two interpretations better represents Kant's intention—Buchdahl suggests that our conclusions probably depend as much on one's own philosophical dispositions as on the content of Kant's arguments. Buchdahl, though, seems to lean towards treating the arguments as analogues to those that operate at the level of reason in Kant.

[98] I generalise to laws of motion here because the other derivations too involve a series of assumptions. See Buchdahl (1969, pp.678-81) for details.

Now, there is a considerable amount at stake here. The purpose of this section is to try to clarify precisely the role of constitutive principles in reason. The account sketched so far is that the categories provide the most general conditions of possibility, which are made particular by addition of a specific experience. In the previous section we saw that a quite convincing case can be made for treating Kant's argument in the "Metaphysical Foundations of Dynamics" in this fashion. The laws of motion are meant to correspond to the categories of relation: if reasoning characteristic of that involved with the faculty of reason is required here, then it would seem that the constitutive principles do not get us as far as we had thought.

Buchdahl's suggestion, then, is that rather than being intended as a deductive proof of the second law, Kant's proof is really intended just to make the law 'rational'. On Buchdahl's account there are three different criteria for the legitimacy of a hypothesis:[99] a hypothesis can be legitimate if it is probable, possible or rational. Might Kant have just intended to make his version of the second law rational?

The criterion of rationality is best defined in contrast with the criterion of probability. For Kant, empirical support can only ever make a hypothesis more probable and, in general, is of little importance in science. This, claims Buchdahl (p.515), though, is only intended to apply to attempts to compare the deductive consequences of single hypotheses with observation. However, this is a simplistic view of scientific procedure: the proper aim of science is to incorporate hypotheses into the framework of a theory. When a hypothesis is incorporated into the pre-existing framework of a theory, the hypothesis is made rational.

Now, the reason that incorporation into a theory's framework makes a hypothesis rational—rather than just more plausible—is to do with the nature of the faculty of reason in Kant's system. Recall that one of the regulative principles that governs reason is a drive towards systematicity and unity, Kant expresses this thus:

> If, then, it can be shown that the three transcendental ideas (the psychological, the cosmological and the theological), although they do not directly relate to, or determine, any object corresponding to them, nonetheless, as rules of the empirical employment of reason, lead us to systematic unity...and that they thus contribute to an extension of empirical knowledge without ever being in a position to run counter to it, we may conclude that it is a necessary maxim of reason to proceed always in accordance with such ideas. This, indeed, is the transcendental deduction of all ideas of speculative reason...as *regulative* principles of the systematic unity of empirical knowledge in general, whereby the empirical knowledge is

[99] See (Buchdahl, 1969, pp.512-6) for a detailed account.

> more adequately secured within its own limits and more effectively improved than would be possible...through the employment merely of the principles of the understanding. (A671/B699)

So, for Buchdahl:

> [A] hypothesis is legitimised as rational because it is a creature of reason; a reason which, when expressed as the system of 'regulative principles of the systematic unity of the manifold of empirical knowledge in general', is what Kant calls a 'transcendental presupposition' of the very possibility of a theoretical system itself. (Buchdahl 1969, p.516)

The force of Buchdahl's charge should now be clear. Rather than intending the principle of inertia to be derived deductively from the category of causation and dependence, the "proof" is rather intended only to gesture at a means by which we can attain greater systematic unity by treating the principle of inertia as, in hindsight, related to the categories.

To clarify the situation, here, it is helpful to emphasise a fundamental disagreement between Kant and Newton. Newton's physics is based upon the idea that absolute motion is motion with respect to absolute space: this is a three-dimensional Euclidean structure within which the motions of bodies satisfy the laws of motion. Newton's most fundamental problem is that in our experience of nature we are given neither absolute space nor, for example, force free bodies. As such it is not entirely clear how it is that we are meant to distinguish the true motions from merely apparent motions. This, in effect, is the goal of the universal law of gravitation: it eventually settles the question as to whether the earth orbits the sun or vice versa by demonstrating that they both in fact orbit around a point between them.

Kant, of course, cannot view the purpose of the universal law of gravitation in this fashion because he has rejected the existence of absolute space. The problem for Kant, instead, is as follows:

> But if the movable, *as such a thing*, namely with respect to its motion, is to be thought of as determined for the sake of a possible experience, it is necessary to indicate the conditions under which the object (matter) must be determined in one way or another by the predicate of motion. At issue here is not the transformation of semblance into truth, but of appearance into experience; for in the case of semblance, the understanding with its object-determining judgments is always in play, although it is in danger of taking the subjective for the objective; in the appearance, however, no judgment of the understanding is to be met with at all. (2004, pp.93-4; 4:555)

Friedman interprets this passage as claiming that the derivation of true motions from merely apparent ones is a matter of *constituting* experience from appearance (1992a, p.142). This, I think, makes good sense of Kant's claim here. It explains why he does not consider the task as transforming semblance into truth: semblance, he says, already incorporates the understanding in a way that is absent from mere appearance. What is to be explained is not just that there is this given appearance, rather that it can be considered an *experience*—which, as we saw, in §2.2 means bringing mere intuitions under categories.

Understanding Kant's intention in this fashion, in effect, inverts Newton's argumentative structure: whereas Newton started with absolute space and argued towards the true motions of the solar system, Kant "conceives this very same Newtonian argument as a *constructive procedure for first defining the concept of true motions*" (Friedman 1992a, p.143). Kant's aim is not to find the true motions: rather it is to understand how it is that the concept of a true motion has objective validity. So, the question is as to how it is possible to have agreed upon the true motions of the solar system in such a way that everyone can accept it.

This is where the laws of motion come in on Friedman's account: they are the conditions under which the idea of true motion has meaning:

> Kant…views the laws of motion as definitive or constitutive of the spatio-temporal framework of Newtonian theory, and this, in the end, is why they count as a priori for him. Using laws of motion we do not then find, discover, or infer that the center of mass of the solar system is in a state of absolute rest; rather the center of mass of the solar system yields that frame of reference wherein the concepts of true or absolute motion are *defined*. (*ibid.*)

Now, recall that in §2.3, we saw that Kant suggested that the dynamical categories were in some sense regulative, so it should not necessarily come as a surprise that we find the laws of motion derived from these categories as seeming to also exhibit a dual status.

If there is a role for reason, then it is in making the laws of motion rational. That they are involved at this stage does not obviously preclude the laws of motion from playing the constitutive role that Friedman assigns them. The important lesson is that even in Kant's system the scope of what purely constitutive principles could achieve was limited. As the understanding stretches into the empirical realm regulative principles are required to provide more specific detail to the constitutive principles. Understood in the manner outlined above, this does not, though, obviously undermine the constitutive status of the pure concepts of the understanding after they have been transformed into principles of pure natural science by the demands of reason.

2.6. A regulative or constitutive reading of Kant?

In this chapter I have argued that a proper understanding of Kant's philosophy of science must pay due attention to the role of both constitutive and regulative principles. I have considered two arguments from the *Metaphysical Foundations of Natural Science.* First, I considered Kant's account of matter: in particular his claim that it fills space by a repulsive force and is necessarily equipped with an attractive force as well. Second, I examined his account of the derivation of the universal law of gravitation. In both cases, I have argued that Kant's account requires both constitutive and regulative principles.

With regard to Kant's theory of matter I have argued that the empirical basis for Kant's claim is very minimal: it requires only the observation that matter resists penetration. I have argued that Warren is correct in arguing that Kant was using a pre-Newtonian conception of force in this chapter (where force is taken to affect the configuration of bodies only) and, as such, the argument is not entirely convincing from the modern perspective. Nevertheless when the argument is treated in this fashion it permits a quite clear view about the particular role of the understanding at this stage of Kant's philosophy of science. The mathematical category of quality provides the most general conditions for the possibility of matter; however, this is only suitable to function as a principle of natural science once a role for experience is also specified. The constitutive principles of the understanding are not enough on their own to explain the possibility of Newton's mathematical natural science.

Matters become more complicated still when we consider the dynamical categories. Here it seems that Kant's derivations of the principles corresponding to the relevant pure concepts of the understanding is much more difficult to understand in an appropriately deductive fashion. Buchdahl, I suggest, is correct in claiming that the derivations of the laws of motion are best understood as providing rationalisations for the laws: as such they seem to be appealing chiefly to the regulative ideal of systematicity. However, given how Kant intends to use these principles, as conditions under which the notion of true motion has objective meaning, this does not necessarily undermine his objective. Instead it seems that regulative and constitutive principles work in concert: the regulative principles pursue systematicity while the constitutive function of the understanding shows how the laws thus derived may function as providing objective meaning to empirical science.

As we have seen, Kant's philosophy of science can no longer be plausibly defended in its original form. Indeed, given that Kant's philosophy was developed by means of an investigation into the conditions of the possibility of natural science, it would be somewhat remarkable if his philosophical framework survived major revolutions in physics. Friedman argues that we can retain a sense in which constitutive principles are synthetic and a priori and it is these that ultimately allow for the construction of an objective science: the constitutive principles are shared by all practitioners and the laws of a theory are deduced from them. However, I would suggest that the doctrine of the synthetic a priori emerges from the nature of the mathematics and natural science that Kant had available to him in the eighteenth century: e.g., with the contemporary understanding of mathematics, it would not seem natural to conclude that geometrical judgments are synthetic a priori. I suggest, then, that we ought to approach the question of the possibility of the objectivity of science in a quite different manner: i.e., by reconceiving Kant's schematism in the manner advocated by Cassirer. This, as we will see in §3.3.2, involves emphasising the role of regulative principles in Kant's philosophy.

The argument of this chapter is important because it shows that both constitutive and regulative principles played an equally important role in Kant's explanation of the objectivity of science. This means that a contemporary Kantian philosophy of science that emphasises either constitutive or regulative principles has equal claim to be "properly Kantian". Friedman objects that a regulative approach cannot do justice to Kant's distinction between constitutive and regulative principles, as such, he suggests, the constitutive approach is to be preferred. In part, the task of the following chapters is to show that a regulative Kantianism can make sense of this distinction. I would suggest, however, that in seeking to retain a role for the synthetic a priori, Friedman does not do justice to Kant's key insight: i.e., that philosophy of science should be concerned with the critical question as to the possibility of the objectivity of mathematics and natural science.

In the next chapter I examine Cassirer's regulative reading of Kant. I argue that by placing emphasis on regulative principles, Cassirer is able to develop plausible answers to both CR and CC. Furthermore a regulative Kantianism has room to incorporate Friedman's most important insights into the nature of the relativized a priori. The main differences are the denial of the syntheticity of the a priori and the manner in which the objectivity of science is secured. So, we will see that as it was for Kant, so it must be for contemporary Kantianism: constitutive and regulative principles must work together in order to explain the objectivity of science.

3

Reason, objectivity and structure

Regulative principles in Cassirer's philosophy of science

3.1. A role for regulative principles in mathematical science?

In the previous chapter I introduced the constitutive and regulative readings of Kant's philosophy of science in the context of Kant's attempt to provide metaphysical foundations for Newton's law of gravitation. Friedman, we saw, argues that Kant should be understood as grounding the law of gravitation in the constitutive function of the understanding, while Buchdahl argues that it is the regulative function of reason that plays the crucial role. I argued that that regulative and constitutive principles work in concert: the regulative function of reason pursues systematicity while the constitutive function of the understanding shows how the laws thus derived may function as providing objective meaning to empirical science. The aim of this chapter is to extend this conciliatory reading and to lay the foundations for my own account of scientific development that seeks to do justice to the intuitions of both constitutive and regulative readings.

In the introduction I outlined two challenges that faced any Kantian account of the philosophy of science: the challenge of rationality (CR) and the challenge of constitutivity (CC). CR asks how the Kantian should provide an account of the rationality of scientific theory change; CC asks how the Kantian should retain something of Kant's idea of constitutivity given the threat that modern mathematics and physics poses to his idea of pure intuition. In §1 I detailed Friedman's constitutive approach to meeting these challenges: my main objection to his approach being to his attempt to retain a sense of the *syntheticity* of the constitutive a priori. I suggest in the following chapters that certain features of Friedman's account—especially his historicisation of the constitutive a priori—can be profitably understood within the framework of a regulative Kantianism.

Friedman contrasts his position with Cassirer's account of philosophy of science, which he argues provided a purely regulative account of science. By this Friedman means that what is ultimately a priori for Cassirer can only be known at the end-point of scientific inquiry: this idealised end-point serves as a regulative ideal that constitutes the entire sequence of scientific theories. Friedman is quite right that Cassirer emphasised the role of the regulative a priori in his account of science and he is also right that without some account of constitutive principles, Cassirer's approach is unsuited to satisfactorily explain the development of science. In this chapter I offer an alternative reading of Cassirer's philosophy of science that places greater emphasis on the idea of constitutivity than Friedman's reading does. I argue that Cassirer understood constitutivity in two ways. The most well-known is his law-constitutive account of objects, which is the claim that the laws of physics make possible the objects of physics. I argue that Cassirer was also committed to a historicised account of the constitutive role of physical principles in the development of laws that is similar to that defended by Friedman. In this way I hope to be able to develop answers to CR and CC that incorporate features of both Friedman's constitutive approach and Cassirer's regulative approach.

For Cassirer, the most important aspect of Kant's philosophy was the critical methodology: i.e., that our philosophy of science must be address the transcendental question as to the possibility of the objectivity of science. This means that if we are to begin to understand Cassirer's philosophy of science, we must first have a clear idea as to the scientific theories that Cassirer took to be the subject of his investigation. To this end, I begin this chapter with a discussion of the physics of Helmholtz and Hertz, which were the state-of-the-art scientific theories that Cassirer studied in his first work on the philosophy of science, *Substance and Function*.

The chapter is structured as follows. In §3.2.1 I give an account of the pertinent aspects of Helmholtz's philosophy. I examine the physiological origins of his theory of signs and explain how his account of geometry is, in part, a response to an epistemological problem that grows out of his physiological work. I also emphasise the role of the law of causality in Helmholtz's work and draw attention to the problem of validity that was considered to be among the most significant philosophical difficulties faced by the account. In §3.2.2 I show that Hertz developed his understanding of physics with the intention of resolving the problem of validity in mind: he took the problem as being connected with Helmholtz's failure to consider unobservable mechanisms that may lay behind observable phenomena. I explain how Hertz alters Helmholtz's theory of signs and replaces it by introducing a distinction between a theory and its model. I then briefly examine his account of mechanics, with a view to showing how this addressed the problem of validity.

In §3.3 I turn to Cassirer's work. Before it is possible to provide Cassirer's answers to CR and CC, it is important to clarify how Cassirer understood the substance-theory of concepts and the function-theory of concepts. Understanding this distinction is crucial if we are to understand his account of rationality and constitutivity: it is a proper understanding of the function-theory of concepts that permits us to see the sense in which Cassirer can be meaningfully Kantian while rejecting the distinction between sensibility and understanding. With this in place I then turn my attention to detailing Cassirer's answers to CR and CC. Cassirer's answer to CR relies upon several aspects of the regulative a priori: the idea of ultimate invariants of experience, principles of theory selection and an idea of systematic unity. I argue though that this account does not accurately capture historical examples of theory change: for that we must emphasise more than Cassirer does that theory change took place at the level of constitutive principles. So, as part of my account of Cassirer's answer to CC I argue that there is a role within his system for constitutive principles in providing physical content to physical laws. However, there is another feature of Cassirer's answer to CC: he argues that the objects of a theory are constituted by the laws of a theory.

3.2. Kant naturalised, objectivity lost

If we are to understand Cassirer's philosophy of science, we must first understand the scientific developments to which Cassirer was responding. In this chapter I consider two of the most important scientific developments that influenced Cassirer's philosophy of science: Helmholtz's account of perception and geometry and Hertz's model-theoretic approach to classical mechanics. Helmholtz developed a naturalised account of spatial intuition, which was intended as an alternative to Kant's normative account.[100] However, with this came a problem: Helmholtz argued that our perception of objects is entirely subjective in the sense that it tells us only about the nature of our own faculties. This left Helmholtz with a problem: how should he account for the objectivity of science? And what about the objectivity of geometry? He had no convincing answer to this problem. However, the problem would be taken up by Helmholtz's student, Heinrich Hertz. Hertz developed Helmholtz's account of perception and used it as the beginnings of a model-theoretic account of science. Hertz was then able to develop this model-theoretic account so that it would serve to provide an account of objectivity. In §3, we see that turning Hertz's account of objectivity on its head is one of the defining features of Cassirer's account of science: as such, the process by which Helmholtz's naturalisation of Kant's

[100] Kant had argued that space was an a priori form of intuition. By this Kant meant that space does not exist as a thing-in-itself: instead it is just a fact of the form of human intuition that sense-impressions are given in the manifold of intuition as being spatially related.

work leads to Hertz's understanding of objectivity is an important first step to understanding Cassirer's philosophy of science.

3.2.1. Helmholtz on spatial perception

Helmholtz's central philosophical claim was that sensations should be understood as *signs* of the external world: i.e. sensations are effects that the external world produces in human sensory organs that should not be understood as bearing any relationship of similarity to the external cause of the sensation. This epistemological claim is central in understanding both Helmholtz's account of geometry and his understanding of causation: the task of this section will be to clarify the theory of signs and to detail the impact that it had on Helmholtz's account of geometry.

3.2.1.1. The theory of signs

The theory of signs has its origin in the physiological methodology that Helmholtz had learned from Johannes Müller. Indeed, in his (1977b) Helmholtz introduces the theory in connection to the work of his teacher:

> Regarding the qualities of sensation, Locke had already established a claim for the share which our corporeal and mental makeup has in the manner in which things appear to us. In this direction, investigations into the physiology of the senses, which were in particular completed and critically sifted by Johannes Müller and then summarized by him in the law of *specific energies of sensory nerves*, have now brought the fullest confirmation. (pp.118-9)

Helmholtz, then, saw Müller's physiological investigations[101] as supporting Locke's account of the qualities of sensation. The aspect of Locke's philosophy that Helmholtz seems to be referring to is the distinction between primary and secondary qualities.[102] Primary qualities are spatial and temporal properties; secondary qualities are those such as colour that were thought to have their origin in the perceiving subject rather than in the external object

[101] Müller's most significant contribution to the physiology of sight, as Helmholtz indicated, was his construction of the law of specific sense energies. This was intended to replace the projection theory of the physiology of perception that was prevalent in the early nineteenth century, which Müller showed could not explain stereoscopic binocular vision. Müller's alternative theory of perception, which could account for stereoscopic binocular vision, consisted of two claims. First, Müller suggested that the two retinas should be understood as consisting of pairs of corresponding points. Second, was that doctrine which we have already seen Helmholtz refer to as the law of specific nerve energies. This is the claim that each type of nerve—e.g. optic nerve—is capable of only one type of response to any external stimulation. So, in the case of the optic nerve, whether it is stimulated by light, electricity or pressure it responds by giving the impression of light, darkness or colour.

[102] This is certainly the aspect of Locke's philosophy that Schlick, in his notes accompanying the paper, takes Helmholtz to be referring to.

itself. However, Helmholtz here cannot be committing himself to drawing the same distinction as Locke: he is quite clear that he viewed spatial properties as subjective properties in precisely the same sense that colours are (1977b, p.124). The important lesson of Müller's physiological investigations was that it is the internal organisation of a nerve-type that is responsible for received images and not whatever the external cause of the stimulation is.[103] It is in this sense that Müller's work confirms Locke's philosophy: not in the sense that it justifies a distinction between primary and secondary qualities, but in the sense that it provides evidence that secondary qualities are purely subjective.

The sense in which Müller's law of specific sense energies raises epistemological concerns should be immediately clear: if the nature of our sense impressions depends solely upon our own constitution, how can we hope to have knowledge of the external causes of our sense impressions?

Hence, we see Helmholtz summarise the fundamental question of epistemology in the following fashion:

> What is true in our intuition and thought?[104] In what sense do our representations correspond to reality?[105] (1977b, p.117)

Helmholtz viewed both philosophy and natural science as being chiefly concerned with addressing precisely this question. However, while they have the same concern, the two fields approach the matter from opposite directions. Philosophy, for Helmholtz, "considers the mental side" (*ibid.*): its aim is to discover how the mind works and philosophy proceeds by seeking to separate out from our knowledge all that which has its origins in the external world. The task of science, by contrast, is to discover the laws that govern the external world; it proceeds by seeking to identify and separate off those aspects of knowledge that are brought by the mind.

Helmholtz does not, I think, see himself as partaking in philosophy. He seeks to explain how the natural world functions by identifying what aspects of our experience are

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103 This is how Helmholtz (1977b, p.119) describes the significance of Müller's theory.

104 The German here reads "Was ist Wahrheit in unserem Anschauen und Denken": "Anschauen" can be translated as either "intuition", as it is here, or as "sensation", as it is in Hatfield (1990). Helmholtz seems to take there to be a distinction between sensation and intuition. This is suggested by his claim that "Die Qualitten der Empfindung also erkennt auch die Physiologie als blosse Form der Anschauung an" (1903, p.223): i.e. the "qualities of sensation" are "forms of intuition", Helmholtz uses "sensation" to refer to the actual physical process of perception whereas intuition seems to refer to the ability to receive impressions of the external world more generally. This is intended to stress only that there is a distinction and that this fact suggests that it is preferable to translate *Anschauen*, here, as "intuition".

105 Schlick (1921, p.163) points out that the first of Helmholtz's two questions is poorly phrased because the concept of truth should strictly only be applied to propositions. So, the former question is sensible only insofar as it inquires as to the truth of statements about our intuition. For this reason I will treat Helmholtz's rephrased version of the question as more accurately reflecting his intent.

brought by the mind and then asking what we can say of the natural world. This is particularly clear, as we will see in the following section, in Helmholtz's discussion of geometry. Helmholtz's answer to this epistemological question followed on from Müller's physiological studies: Helmholtz claimed that "sensations, as regards their quality, are only *signs* whose particular character depends wholly upon our own makeup" (1977b, p.122). For Helmholtz this meant that visual perception was purely subjective: i.e., just as for Müller, every sensation is dependent upon our own physiological make-up and not on the external world.

This left Helmholtz with a quite serious problem about the nature of geometry. In the nineteenth century geometry-proper was understood as relating to the structure of the external world. Helmholtz's answer to the epistemological problem in general can, therefore, be understood through his answer to how our spatial representations correspond to the geometry of the physical world. In the next section I examine Helmholtz's understanding of geometry to show how, by separating off the contribution to knowledge made by our mind, we are able to successfully measure the geometry of space.

3.2.1.2. Geometry, the problem of validity and the causal principles

Helmholtz's theory of perception meant that he had to explain how geometry was applicable to both *physical* and *visual* space. Because human perception of objects depends on our physiological constitution, and not the constitution of the external world, it is mysterious both that, e.g., Newton can appeal to Euclidean geometry in deriving physical laws and that our visual space is Euclidean. Helmholtz's answer to this problem provides a model by which we can hope to learn about the external world, even while our visual perception of it is entirely subjective: in short, he advocates nativism with respect to visual space and argues that we can learn about physical space through postulating the existence of rigid bodies which we can physically interact with and experiment on to learn about physical space.

Helmholtz, it should be noted, took himself to be advocating an essentially Kantian position in that he understood space to be a subjective, necessary and given form of intuition. Of course, the details of this account are quite different to the details of Kant's account. The Euclidean structure of visual space is subjective in the sense that human perception of space depends on our constitution; it is necessary in the sense that we necessarily connect perception of spatial relations to an external world; it is given just because perception of spatial relations is not learned, but is given to us by our "makeup".[106] The key difference is that Helmholtz does not take physical space to be necessarily Euclidean. Kant, he suggests, made this additional step only because non-Euclidean geometries were still thought to be logically impossible at the time Kant was writing.[107]

Let us now set out Helmholtz's account of geometry in more detail. Helmholtz understands the Euclidean method as being based on demonstrations of the congruence of lines, angles, bodies etc. This process, in turn, depends upon our ability to imagine the geometrical bodies being moved in thought—without changing their dimensions—so that they are next to each other and can be compared. This process, Helmholtz argued, is not enough on its own to establish the necessity of Euclidean geometry:

> But if we want to erect necessities of thought upon this assumption, that fixed spatial structures can be moved freely without distortion to any location in space, then we must raise the question of whether this assumption involves any presupposition which has not been logically proved. We shall soon see below that it does in fact involve one, and indeed one of far-reaching implications. But if it does, then every congruence proof is supported by a fact drawn from experience. (1977a, pp.4-5)

This is the central claim in Helmholtz's argument against the necessity of treating physical space as Euclidean. In applying Euclidean geometry to space, we assume that we can move bodies around in space, without distortion, precisely as we can in thought. If Euclidean geometry is to necessarily apply to physical space, then the assumption that bodies behave in space as they do in thought must itself be necessarily true. Helmholtz suggested that this is actually an empirical assumption.

Why this is so becomes clear in Helmholtz's analysis of Riemannian geometry. Helmholtz's (1977a) was intended to establish two propositions: first, that Kant's treatment of geometrical axioms as being true of necessity is "at variance with facts" and, second, that there are two types of equivalence of spatial relations and magnitudes—subjective and objective. This distinction corresponds to visual space and the physical space to which we do not have direct epistemic access.[108]

By claiming that Kant's view of geometry is at variance with facts, Helmholtz just means that new forms of geometry have been developed that render non-Euclidean

[106] Helmholtz discussed this in his (1977b, p.124)
[107] This is especially clear in the following passage: "When Kant asserted that spatial relationships contradicting the axioms of Euclid could never in any way be represented, he was influenced by the contemporary states of development of mathematics and the physiology of the senses, just as he was thus influenced in his whole conception of intuition in general as a simple psychic process, incapable of further intuition" (1977b, p.129).
[108] Helmholtz states explicitly that this was the twin aim of his (1977a) and the beginning of his (1878), which is a response to Land's criticism of his original argument.

geometries perfectly conceivable. Helmholtz thought that the reason that Kant took Euclidean geometry to be necessarily true was because in Kant's time there was no conceivable physical alternative. Helmholtz appeals to the work of Beltrami and Riemann to demonstrate the conceivability of non-Euclidean geometry.

Riemann's formulation of geometry showed that the various possible geometries could be recovered from consideration of the notions of position and relations of position (distances). Riemann thought that in order to reflect our ignorance of the nature of space, we should seek to treat it in the most general possible terms: this meant treating space as a manifold upon which relations could be internally imposed to give the surface a variety of geometrical structures.[109] In this way he was able to provide a general abstract mathematical structure that was capable of describing Euclidean space and an incalculable variety of non-Euclidean spaces. Using only ideas of position—the manifold—and relations of position—the metric—Riemann, then, was able to both re-describe Euclidean geometry and point out a way to develop new geometries.[110]

Riemannian geometry seems to have played two roles in Helmholtz's argument. First, Riemann had developed a geometry that had the structure to explain both Euclidean and non-Euclidean geometry. This, in effect, enabled us to conceive of geometries other than Euclidean geometry by treating space as curved. Helmholtz argued that this is an analytic process:

> I…wish to emphasise here that this so-called measure of curvature of space is a calculated magnitude obtained in a purely analytic way, and that its introduction in no way rests upon insinuating relationships which would only have a sense as ones intuited by the senses. (1977a, p.13)

Helmholtz, then, argued that the conceivability of non-Euclidean geometry is entirely analytical. At this stage he is not dealing explicitly with the problem of physical geometry, or space as perceived by the senses. This part of his argument is concerned solely with showing—against the received wisdom of Kant's time—that non-Euclidean geometries are conceivable.

The second part of Helmholtz's argument was related to physical geometry: he claimed that physical geometry can, in fact, be empirically determined to be non-Euclidean.

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[109] For a more detailed account of Riemannian geometry see (Gray, 1979).

[110] Gray provides a neat summary of Riemann's position: "Riemann gave a wholly novel answer to the question: what is geometry? To him geometry was to do with concepts like length and angle which could be intrinsically defined on a surface or space of some sort. It follows that there are many geometries, one for each kind of surface and each definition of distance: a geometry arises from anything in which it makes sense to talk of a distance between two points, and this geometry will have a set of theorems associated with it" (2007, p.193).

As part of his argument for this, Helmholtz considers Riemann's crucial claim that "the essential basis of any geometry is the expression giving the separation between two points for any arbitrary direction of separation" (1977a, p.13). Helmholtz argues that whereas Riemann takes this claim simply as an axiom from which the theorem on the mobility of spatial structures can be derived, the axiom should instead be understood as being derived from three more fundamental presuppositions (p.15).

First, it is presupposed that the situation of any point A is specifiable by coordinates, Second, in order to be able to compare bodies by congruence, it must be presupposed that, for any fixed body, there must be an equation that describes an unchanged spatial relation for any two points of the fixed body for any motion of the body. Thirdly, it needs to be specified that a 360° rotation of any fixed body will restore the body to its initial position. These conditions are supposed to be consistent with our experience of rigid bodies; they are not taken to be necessary.

The second part of Helmholtz's argument depended crucially on showing that, with these three presuppositions granted, we are able to measure non-Euclidean geometries. This part of his account of geometry is also central to understanding his answer the general epistemological question that he set out in his (1977b). The crucial step in his argument was to claim that there do actually exist rigid bodies that are described by the three presuppositions that underlie Riemannian geometry: that is, there are rigid bodies that can be moved around and rotated without distortion. That there exist such bodies can, of course, only be an assumption. However, once it has been granted it becomes a straightforward matter to make geometrical measurements: we can, for example, simply measure the interior angles of a triangle and see whether their sum is equal to, more than or less than 180 and use this to determine which of the three physically possible geometries accurately describe space.

Now, there is a problem with Helmholtz's account as it has been detailed so far. I have stressed in §3.2.1 that on Helmholtz's theory of signs, humans have no epistemic access to the physical world whose geometry Helmholtz sought to determine. However, Helmholtz's argument that we can measure non-Euclidean geometries would seem to rely on our perceiving the results of the measurements: i.e. if we seek to measure the sum of the interior angles of a triangle and find that this comes to more than 180, the measurement is relayed to us by perception. If our sensations are just signs that do not tell us anything of the external world, how is it possible to treat this measurement result as telling us anything about physical space?

Helmholtz sought to explain this by appeal to a principle of causality, stated as follows.

> The only assumption we still maintain is that of the law of causation, to the effect, namely that all mental states having the character of perception that come to pass in us do come to pass according to fixed laws, so that when different perceptions supervene we are justified in inferring therefrom a difference of the real conditions determining them. As regards these conditions—the reality proper that underlies the phenomena—we know nothing. (1878, p.222)

Helmholtz's idea here was that we need to assume that while our sensations themselves do not correspond to an external reality, the causal relationships that we perceive *do* correspond to *causal relationships* in the external world.

The challenge to Helmholtz can be summarised as follows. Helmholtz drew a distinction between the physical world and the phenomenal world—that is, the world as it appears to our senses. Humans have no epistemic access to the physical world because our senses do not perceive any aspect of the external world: sensory impressions tell us only of the nature of the relevant senses—e.g. however one stimulates a retina it gives the impression of colour. However, the task of science remains to tell us of the physical world by seeking to identify and remove those aspects of experience that are subjective products of the mind. This means, among other things, that the scientist should seek to determine the structure of physical space.

Central to Helmholtz's argument that we can determine the physical geometry of space is the idea that elements of experience seem to remain constant while being perceived by different senses. So, consider attempting to measure a particular length with the aim of determining that it is of the same magnitude as a unit measuring rod. You place the unit measuring rod next to the length you seek to measure and see that the two are the same length. You check this by feeling the ends of the two rods to check that they are next to each other, and you can do this with each of your fingers separately.[111]

Helmholtz argued that it would be reasonable to conclude from the consistency of our various different sense-impressions that objects so-checked do actually have the physical properties that they appear to have in the phenomenal world. However, because we have no direct epistemic access to the external world this principle can never be accepted on the basis of anything more than trust. Helmholtz is explicit in accepting this consequence of his theory of signs:

> This is a trust in the lawlikeness of everything that happens. However lawlikeness is the condition of comprehensibility of the appearances of nature. While: should we presuppose that this comprehension will come to completion, that we shall be able to set forth

[111] See Helmholtz's (1977b, p.127).

> something ultimately and finally unalterable as *the cause* of the observed alterations, then we call the regulative principle of our thought which impels us to this the *law of causality*. We can say that it expresses a trust in the *complete comprehensibility* of the world. (1977b, p.142)

At the root of Helmholtz's method for determining the geometry of the physical world is trust in the idea that regularities in the physical world will remain constant and that our senses are capable of identifying these regularities.

This is the source of Helmholtz's disagreement with Kant over what Helmholtz views as Kant's nativism. Helmholtz argued that once it is appreciated that our knowledge of the world is dependent upon this principle of trust, many principles that Kant took as a priori are better understood as empirical. So Kant assumed that we perceive space as Euclidean because that is the structure of our form of intuition. However, for Helmholtz, the Euclidean nature of geometry is learned empirically through the consistent detection of Euclidean regularities in the phenomenal world that are caused by the Euclidean nature of the physical world. For Helmholtz Euclidean geometry appears to be true of necessity because we are psychologically adapted to detecting Euclidean regularities in the external world.

This principle of trust in the lawful regularity of the physical world was the source of an objection to Helmholtz's work—raised by various figures, including Bauch, Cohen and Rickert—known as the problem of validity.[112] This objection is a variant of the problem of induction: it is simply the claim that, given Helmholtz's naturalism, we have no grounds at all to assume that the regularities detected in the world will continue indefinitely. That is, just as we may doubt the claim "all swans are white" on the basis of our observations of a finite number of observations of swans, all of which are white, we ought to doubt the claim that the geometry of space is Euclidean on the basis of a finite number of observed regularities. To establish the validity of Helmholtz's method for determining spatial geometry we are required to make an assumption that goes beyond any possible experience. Helmholtz's naturalism—with its principle of trust in the lawful regularity of the world—certainly does not have a satisfactory resolution to this problem.

The problem of validity plays an important role in this chapter's narrative because one of the factors motivating Hertz's account of classical mechanics was a desire to solve the problem of validity. In the next section I will detail Hertz's response to this problem.

[112] The importance of the problem of validity in understanding both Helmholtz's work and later philosophical responses to it is emphasised in (Patton, 2009).

3.2.2. Hertz's "images of science": objectivity regained?

In an article written for Helmholtz's seventieth birthday,[113] Hertz outlined what he understood to be the most significant achievements of his tutor: the invention of the ophthalmoscope, the development of the principle of the conservation of energy and his work on the physiology of the senses. It is with the last of these that I will be primarily concerned in this section. Hertz took Helmholtz's physiological research to show that the senses mediate between two worlds: the "cold and alien world of actual things" and the "intellectual world of conceptions and ideas" (1891, p.335). The senses are the only means through which communication between the two worlds is possible: i.e. it is only through impacting upon our sense organs that changes in the external world can make themselves felt. This particular characterisation of the relationship between human ideas, the senses and the external world is very much central to Hertz's own philosophy of science. He also noted several questions that Hertz's account of vision had raised and left unanswered, one of which has definite echoes in the *Principles of Mechanics* (2007 [1894]), published three years later:

> Is the manifold of these relations sufficient to portray all conceivable manifolds of the external world, to justify all manifolds of the internal world? (1891, p.336)

That is, do the mental conceptions of the world that we form through the process of the external world impacting upon our senses exhaust the possible conceptions of that world? Helmholtz's theory of signs dealt only with the external world insofar as it is available to the senses; Hertz, here, is concerned with whether that is sufficient, or whether we need to postulate unobservable objects and relations in order to explain experience.

It is in answering this question that Hertz begins to develop an account of *Bilder*.[114] His answer, provided in the *Principles of Mechanics*, is that "the manifold of the actual universe must be greater than the manifold of the universe that is directly revealed to us by our senses" (2007, p.25). Hertz argued that if we attempt to explain the motions of bodies with reference only to that which can be directly observed, we will, in general, fail. For Hertz the central problem with which science should be concerned is with "the

[113] (Hertz, 1891).

[114] This translates as "pictures" or "images". In the English translation of the *Principles of Mechanics,* "Bild" is translated as "image". However, as Patton notes (p.281n)—translating the word as "image" or "picture" may give the impression that a *Bild* should be understood as a mental image or picture, which is potentially misleading.

anticipation of future events" (p.1).[115] Now, if we concern ourselves solely with what can be observed, it proves impossible to accurately make future predictions. This means that our *Bild* of the universe must make appeal to unobservable objects and/or forces to ensure successful prediction of future events.[116]

From a contemporary perspective, then, Hertz's *Bilder* are best understood as models. They are intended to provide accurate predictions about the behaviour of observable objects and they do so by positing hypothetical entities and mechanisms that underlie observable phenomena and make successful predictions possible. In Hertz's (2007) *Bilder* is translated as "image". However, since Hertz employed the term to refer to what we would now clearly understand as models, and "image" has potentially confusing connotations, I will translate *Bild* as "model" throughout. Hertz contrasted models with "symbols of external objects":

> We form for ourselves images [*innere Scheinbilder*] or symbols of external objects; and the form which we give them is such that the necessary consequents of the images [models, *Bilder*] in thought are always the images [models, *Bilder*] of the necessary consequents in nature of the things pictured. (*ibid.*)

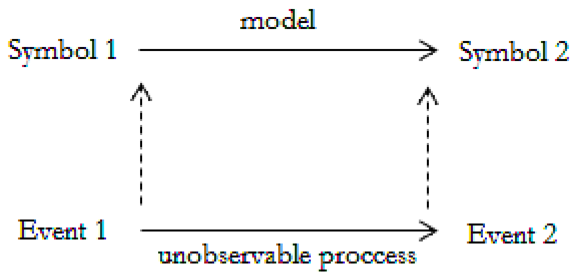
There were two types of "image" in the *Bildtheorie*. First, there are what Hertz referred to as *innere Scheinbilder* or symbols.[117] These are the impressions that are produced in us by means of external objects impacting upon our senses. These symbols, I suggest, are essentially the same as Helmholtz's signs: they are just external objects as they appear to us as mediated through the senses. However, as we have seen, Hertz does not think that this alone is sufficient to provide knowledge—understood in terms of predictive success—of the external world. In order for this a second, model-theoretic, understanding of *Bilder* is introduced: it is these models that are of primary interest in Hertz's *Bildtheorie*. Hertz understood models as theoretical frameworks that aim to accurately predict the causal behaviour of actual objects. So, the purpose of a model is just to accurately predict the behaviour of the observable world—as it appears to us as symbols—and to do so by postulating various unobservable entities whose behaviour is governed by hypothetical laws. This can be represented pictorially as follows:[118]

[115] It is with this idea, that the purpose of our conscious knowledge of nature is to enable the anticipation of future events, that Hertz opens his (2007): as such, it is clear that it is a vital aspect of Hertz's account of the nature of science.

[116] "If we wish to obtain an image [*Bild*] of the universe which shall be well-rounded, complete, and conformable to law, we have to presuppose, behind the things we see, other, invisible things—to imagine confederates concealed beyond the limits of our senses" (1894, p.25).

[117] Henceforth, for clarity, I will refer to *innere Scheinbilder* as "symbols".

[118] This diagrammatic representation of the *Bildtheorie* is adapted from van Fraassen's (2008, p.196).



External events are observed and represented as symbols. As we have seen, Hertz argued that the symbols that we form are such that the "necessary consequents of the images in thought are always the necessary consequents in nature of the things pictured". Consider the following simple two events: (1) a moving body impacts a stationary body and (2) the stationary body starts moving and the velocity of the body striking it is decreased. We then create symbols that correspond each of these two events. Now, these symbols need to be related to each other so that Symbol 2 is a necessary consequent of Symbol 1. This requires a model that aims to capture the unobservable process that occurs between Event 1 and Event 2. In the simple case of two colliding objects, this model would appeal to concepts such as mass, velocity and elasticity to ensure that the representation of Event 2 captures the velocity at which the two bodies are symbolised as moving. The problem is that we can perfectly well form symbols of observable events, but we need to ensure that the symbols that we make are related as cause-to-effect and this is the role of models. Hertz's further step is to assume that the models that explain how each of our symbols is a necessary consequence of earlier symbols represent the actual unobservable process between Event 1 and Event 2.

In the discussion of Helmholtz's sign theory I stressed what has been called the problem of validity: how can Helmholtz justify the assumption that the world-as-it-appears will continue to display the same set of regularities for all time. Helmholtz, we saw, was not entirely clear on how this problem was to be avoided: in his (1878) he seemed to treat the stability of observed regularities as a matter of trust, whereas earlier in his (1977a [1868]) he had argued for a species of psychological adaptationism. As Hertz's account has been sketched so far, one might think that precisely the same objection applies to Hertz's model-theoretic account: what guarantees that our models accurately represent unobservable processes? Hertz, though, provided a much more compelling solution to this problem than Helmholtz. Hertz's solution to this problem can be found in his *Electric Waves* (1893).

Hertz's account of models was closely related to his understanding of precisely what a scientific theory is. Hertz discussed this in relation to Maxwell's theory of electromagnetism, famously—if somewhat misleadingly—declaring that the answer to the question "What is Maxwell's theory?" is: "Maxwell's theory is Maxwell's system of equations" (1893, p.21). This, understandably, has been taken to demonstrate that Hertz understands a scientific theory to amount to its mathematical expression in the form of sets of equations. However, to draw this conclusion would be mistaken.

Prior to posing this question, Hertz had described how he learned electromagnetism (p.20). Here he admitted great admiration for Maxwell's "mathematical conceptions" while confessing that he was unsure of the physical significance of Maxwell's work. For this reason, Hertz had hoped to be able to derive Maxwell's equations by using a limiting case of Helmholtz's electromagnetism that would give the equations a more sure physical grounding.[119] Hertz had studied electromagnetism under Helmholtz and had practiced in accordance with Helmholtz's version of the theory. As Buchwald (1994, ch.6) demonstrates, this involved incorporating certain Helmholtzian concepts into his experimental practice: as such, it should be no surprise that Hertz was more comfortable with the physical significance of Helmholtz's electromagnetism than he was with the physical significance of Maxwell's. However, as Hertz noted in his (1893), Helmholtz could never firmly banish action-at-a-distance from his theory, and as soon as this is recognised the physical significance of Helmholtz's theory is not clearer than that of Maxwell's. This situation led Hertz to revise Maxwell's theory again in his (1893); he aimed to keep Maxwell's equations, but leave out as much of the mathematical confusion that he could.

Central to all three versions of electromagnetism, though, were Maxwell's equations. These, Hertz described as the "undying part of Maxwell's work" (p.21). It is at this point in his discussion that Hertz stated that Maxwell's theory is just his set of equations. Hertz goes on to clarify that what he means by this is just that in order to provide the same description of observable reality as Maxwell, one must provide the same set of equations. Hertz does not intend to designate both the equations and the account of their physical significance as Maxwell's theory: Maxwell's theory instead describes just the behaviour of that which is observable. Hertz does not wish to dispute this aspect of Maxwell's account: Maxwell's equations, he thought, correctly predict the behaviour of observables. Hertz was quite clear that his objection was only to Maxwell's account of what

[119] Helmholtz was a central figure in recognising the importance of Maxwell's electromagnetism and in introducing it to the German-speaking world. He initially viewed the theory as an argument against Weber's account of electrodynamic interaction, which was based on the interaction of moving charges. Weber's account was considered problematic because it seemed to require a violation of energy conservation; Helmholtz saw in Maxwell's theory—with its postulation of the contiguously-acting aether—the possibility of avoiding this problem. Helmholtz sought to develop his own theory of electromagnetism with a view to deciding between Weber's action-at-a-distance theory and Maxwell's contiguous-action theory. Helmholtz's attempts in this direction were not wholly successful—he was not able to derive Maxwell's equations under all conditions—and Maxwell's theory was near-universally preferred: however, Helmholtz's own version of electromagnetism was able to recover Maxwell's equations under limiting conditions. For detailed discussion of Helmholtz's electrodynamics see (Kaiser, 1993) and (Buchwald, 1994, Appendix 16).

underlies his equations. His intention was to provide an alternative representation (*Darstellung*) of Maxwell's theory that would predict precisely the same observable outcomes, but physically ground them in a quite different fashion.[120]

It would, then, be more precise to say that Hertz understood a theory to be the mathematical expression of that theory only in the sense that a theory's set of equations provides a stable description of the behaviour of observable regularities and relations. The model-theoretic representation of a theory—that is, the concepts and unobservable mechanisms that it appeals to—is just as crucial in assessing a theory.

The reason for this, I suggest, is that it is the representation of a theory that provides the grounds to avoid the problem of validity. Maxwell's equations, on their own, describe observable relationships between various concepts.[121] On Helmholtz's theory of signs, Maxwell's equations successfully describe observed regularities; however, on Helmholtz's theory alone we can have no reason for thinking that the equations will continue to describe these regularities. In effect Helmholtz's account is faced with a species of the problem of induction: while the equations have successfully described the generation of electric and magnetic fields in the past, how can we be sure that they will continue to do so in the future? The theory of signs struggled with this question because it did not allow for any epistemic access to the external world. Hertz sought to remedy this by outlining a means by which we might have knowledge of the unobservable realm that underlies experience.

Hertz argued that we could judge three ways in which we could test a model in experience:

i. *Permissibility*. This is a question of the logical consistency of a model. If a model is logically consistent it is permissible; if it is logically inconsistent it is impermissible.

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[120] "Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory; every theory which leads to different equations, and therefore to different possible phenomena, is a different theory. Hence in this sense, and in this sense only, may the two theoretical dissertations in the present volume be regarded as representations of Maxwell's theory. In no sense can they claim to be a precise rendering of Maxwell's ideas. On the contrary, it is doubtful whether Maxwell, were he alive, would acknowledge them as representing his own views in all respects." (1893, p.21)

[121] Maxwell's equations describe the relationship between the concepts electric field, magnetic field, charge and current. Assuming that we have reliable measurement procedures that can determine the values of these concepts, then there is a purely phenomenological level upon which Maxwell's equations work: i.e., they provide predictions for the strength of the electric and magnetic fields generate in the presence of each other and certain charges and currents. If we have the appropriate measurement devices we do not need to know the theoretical explanation of why these fields take the value that they do, we can determine that the values accord with Maxwell's predictions by measurement alone.

ii. *Correctness*. This is a question of whether or not a model predicts relations that contradict observed relations: if a model makes predictions that contradict observation then it is judged as invalid.

iii. *Fitness for purpose*. This is a more subjective measure of the validity of a model. It is possible for several models to explain the same phenomena and be correct and permissible. Hertz suggested that we decide between such models by seeking that model which coordinates one relation within the model to one relation in the world.

In the *Principles of Mechanics* Hertz demonstrated how this would work for physics. For example, he argued that while Newton's model of classical physics—based on the concepts space, time, mass and force—was both correct and (probably) permissible, it was not fit for purpose. Hertz considered a solitary bit of iron resting upon a table. Newton's model supposed that there are attractive forces operating between all the atoms in the universe and yet "in spite of a thousand existing causes of motion, no motion takes place" (p.13). This, for Hertz, coordinated one relation in the world (the iron staying at rest on the table) to uncountable relations in a model and, thus, he argued that his alternative model (which is discussed in §3.3.1) could do better. This way of testing a model in experience, then, was how Hertz sought to solve the problem of validity. It gave Hertz a schema that allowed him to argue for the objectivity of models of science, and this objectivity was based on the idea that it was most likely that the concepts and relations appealed to in a scientific model accurately represent concepts and relations in the physical world. Hertz's position, then, represents a clear development of Helmholtz's sign theory. On the one hand Hertz accepted the central part of Helmholtz's claim: that objects appear to us only as signs (or symbols) and that we can have no direct knowledge of the world as it is in itself. However, Hertz also argued that Helmholtz's philosophy was overly restrictive: he wished to argue that we can have a degree of knowledge of the external world by constructing models that predict the responses of the world-as-it-appears to causal interference.

3.2.3. Helmholtz, Hertz and objectivity

The aspect of the philosophy of Helmholtz and Hertz that I have paid most attention to here is the problem of validity. The root cause of this problem was Helmholtz's argument that our sense impressions are purely subjective in the sense that they tell us about the nature of the perceiving subject rather than about the nature of the external world. This subjectivity clashed with the supposed objectivity of our scientific knowledge: e.g., the facts of geometry were thought to be the same for all observers and at all times. However, if our epistemic access to the external world is mediated in the sense that Helmholtz and Hertz understood it to be, how can we possibly secure the objectivity of scientific phenomena? Helmholtz had no convincing answer to this question, relying solely on a principle of trust in the lawful regularity of the external world. Hertz inherited precisely this problem from his teacher. Like Helmholtz, he understood the senses as mediating our epistemic access to the external world, and he too had to explain the objectivity of science. Hertz developed an early form of a model-theoretic account of science in order to solve this problem. On Hertz's account, we create models of science with the aim of capturing causal relationships among observable phenomena. He provided three criteria for choosing between alternative possible models of science and argued that the model that best meets these criteria should be understood as accurately representing physical reality. So, for Hertz, the objectivity of science was secured by assuming that the concepts and processes appealed to in our models of observable reality correspond to actually existing entities in the external world.[122] This account of objectivity is important, as we will see in the next section, because it is this that is eventually turned on its head by Cassirer.

3.3. Cassirer's normative turn: a Kantian account of objectivity

Cassirer detailed his philosophy of science in three main works: *Substance and Function*[123] (1923 [1910], henceforth SF), *Einstein's Theory of Relativity* (1923 [1921], henceforth ETR) and *Determinism and Indeterminism in Modern Physics* (1956 [1936], henceforth DIMP). The task of this section is to clarify the role of the a priori in Cassirer's work. This has recently been the cause of some dispute. Friedman argues that Cassirer identified a regulative and absolute role for the a priori in science (2000, p.115ff); Richardson argues that Cassirer's philosophy of science is best understood as a—losing—struggle to find a role for the relativized and constitutive a priori consistent with the regulative a priori (1998, p.136); Heis agrees with Richardson that Cassirer defended both constitutive and regulative aspects of the a priori, but argues that Cassirer's account does not fail as Richardson suggests (2012); finally, Ryckman suggests that Cassirer defended a version of the a priori according to which certain scientific principles, such as general covariance, are a priori in both a constitutive and a regulative sense (2005, p.46). The end goal of this section is to resolve this dispute and provide clear answers on Cassirer's behalf to CR (§3.2.1) and CC (§3.2.2).

[122] In introducing the problem of validity I pointed out that it was a variant of the problem of induction.

[123] In German, this was published as "*Substanzbegriff und Funktionsbegriff*": it is better translated as *Substance-concepts and Function-concepts*.

However, before we get to this problem, a more fundamental problem must be solved. Cassirer makes an important distinction, in each of SF, ETR and DIMP between the substance-theory of concepts and the function-theory of concepts. The first task of this section will be to make sense of the difference between these two theories of concepts. It is here that the discussion of the work of Helmholtz and Hertz will be particularly helpful to our analysis: this is because in SF Cassirer took both Helmholtz and Hertz to advocate a version of the function-theory of concepts. However, after the developments of general relativity and quantum physics, Cassirer came to understand both as representing the substance-theory of concepts. A discussion of what prompted this change is deeply informative from the perspective of clarifying the distinction between substance and function theory of concepts.

Cassirer's claim that the development of logic, mathematics and science has shown that a substantial theory of concept formation must be replaced by a functional theory of concept formation is central to each of SF, ETR and DIMP: the first half of SF is dedicated to arguing for the emergence of a functional theory of concept formation and in both ETR (e.g., p.392) and DIMP (e.g., p.130) Cassirer emphasised the importance of this theory of concept formation. Indeed, for Cassirer, the philosophy of science just *was* the theory of the formation of scientific concepts. This understanding of the philosophy of science as a theory of concept formation provided Cassirer with a unifying theme that enabled him to draw together the three key aspects of scientific knowledge: logic, mathematics and empirical observation.[124] It also allowed Cassirer to construe the relationship between these aspects of science in a significantly different fashion to Helmholtz and Hertz. Both Helmholtz and Hertz, we have seen, took objects to be of primary epistemological interest: in both cases it was assumed that the world consisted of self-subsisting objects that—because we gain knowledge of the world through our senses—we do not have direct epistemic access to. The task of science was understood as being to develop concepts suitable to providing true descriptions of the behaviour of external objects. Thus, on this view of science, the objectivity of science is a goal that is achieved through providing a true description of the behaviour of objects. Cassirer turns this entirely on its head: the objectivity of science is the fundamental epistemological fact while objects are known only in virtue of their relation to mathematical structures. When the two theories of concepts are understood in this fashion, it greatly illuminates Cassirer's account of both constitutivity and the rationality of science.

124 See Heis (2011b, §1) for a detailed discussion of why Cassirer places so much emphasis on philosophy of science as a theory of concept formation.

3.3.1. *From substance-concepts to function-concepts*

Cassirer organised SF around the theme of theories of conceptual development: Cassirer sought to show that in arithmetic and geometry we are compelled to abandon a substance-theory of concept formation and embrace instead a function-theory of concept formation. Clarifying the precise distinction between these two theories of concept formation is central to understanding Cassirer's account of the a priori. I argue that there were two aspects to Cassirer's understanding of substance-concepts and function-concepts: the first is related to the type of concepts that should be used in science and the second is related to epistemological theory. That is, there are differences in:

1. *Types of concepts*: At this level there is a distinction between substance-concepts and function-concepts in how concepts are understood. So, e.g., at this level on the substance-theory of concepts, a concept such as <natural number> is understood as an individual; on the function-theory of concepts, <natural number> is understood in terms of the "general *logic of relations*" (SF, p.37). At this level, then, the transition from substance-theory to function-theory implies a transition from understanding concepts as individuals to understanding concepts in terms of relations.
2. *Epistemological theory*: At this level, the substance-theory and the function-theory of concepts represent entirely different approaches to epistemological inquiry. The substance-theory treats the concept of <object> as the fundamental entity of epistemological inquiry. On this account the concepts <truth>, <knowledge> and <objectivity> are all explained in terms of the basic concept <object>. However, on the function-theory of concepts, <objectivity> is taken to be the fundamental entity of epistemological inquiry. Instead of seeking to explain how we have objective knowledge of the world, that we do have objective knowledge of the world is taken as primitive and the concept <object> is eventually explained in terms of <objectivity>.[125]

[125] See Mittelstaedt (2009), who argues that an essential component of the Kantian approach is that scientific practice should be understood as beginning with <objectivity> and ending in <object>. This aspect of the distinction between substance-theory and function-theory of concepts is also emphasised in (Heis, 2011b, §2), where the concepts <truth> and <knowledge> are also introduced as mediating between <object> and <objectivity>. Heis, though, focuses his analysis on the mathematical part of SF. I argue that in the part of SF that addressed the development of the function-theory in the natural sciences, it is not clear that Cassirer entirely ruled out an explanatory role for the concept <object>. As we will see, while Cassirer quite emphatically rejected the idea of mathematical objects in SF, he did not as clearly reject physical objects: this, as is clear from the discussion of Hertz and Helmholtz in the previous section was because, at the time, physicists still held to the idea of independently existing objects. It is only in DIMP that Cassirer rejected the physical concept <object> as having any possible explanatory role.

The task of this section, then, is to defend the above understanding of Cassirer's distinction between the substance-theory and function-theory of concepts and to draw out the implications for Cassirer's understanding of natural science.

The concepts <object>, <truth>, <knowledge> and <objectivity> represent different stages in the cognition of objects: the relationship between these stages is understood differently in the substance and function theories of concepts. It was through interpreting the relationship between these concepts in accordance with the functional-theory of concepts that Cassirer sought to historicise Kant's transcendental logic.[126] Cassirer interpreted logicism as implying that fundamental mathematical relations could not be explained in terms of the principle of non-contradiction—as they would have been on Kant's account—and instead had to be understood in terms of function and relation. Cassirer, then, understood this to be a call for a "logic of objectual knowledge" in general:

> [These] same basic syntheses *(Grundsynthesen)* upon which mathematics and logic rest, also govern the scientific structure of empirical knowledge and first enable us, by a fixed lawful ordering of phenomena, to speak of its objective significance. (Cassirer 1907, p.45, translated by Ryckman, 1991, p.65)

This, then, is a demand that we account for objects in terms of a transcendental logic that would reveal the constitutive presuppositions of the concept of object.

It is important to emphasise the distinction between pursuing a Kantian account that places transcendental logic at its core as opposed to a Kantian account that emphasises the synthetic a priori. I take it that there are two key distinctions: first, Cassirer's approach requires that we consider the objectivity of the *entire sequence* of scientific theories and, second, on Cassirer's approach there is no division between the faculties of sensibility and understanding.

I will argue that there is a certain resemblance between Reichenbach's attempt to relativize Kant's a priori and Cassirer's attempt to historicise his transcendental logic: i.e., in that relative a priori principles that play a constitutive role do have a place within Cassirer's system.[127] However, the difference in the approaches is significant. Consider Cassirer's description of his approach in the first volume of *Das Erkenntnisproblem*:

> The task, which is posed to philosophy in every single phase of its development, consists always anew in this, to single out in a concrete, historical sum total of determinate scientific concepts and principles the general logical functions of cognition in general. This sum total

[126] This point is made by Ryckman (1991, pp.65-6).
[127] I discuss this in §3.3.2.

can change and has changed since Newton: but there remains the question whether or not in the new content that has emerged there are some maximally general relations, on which alone the critical analysis directs its gaze, and that now present themselves under a different form and covering. The concept of the *history of science* itself already contains in itself the thought of the *maintenance of a general logical structure* in the entire sequence of special conceptual systems. (Cassirer, 1922, p.16, translation in Heis, 2011a, p.767)

Here, Cassirer began by setting out an idea that is close to Reichenbach's: that there are certain concepts and principles that underlie each stage of scientific development. Cassirer's approach, though, goes beyond Reichenbach's. It is not enough for Cassirer just to identify the underlying concepts, in addition he argues that one must look for "maximally general relations" that can provide an account of how new conceptual content has emerged from the old. That is, in seeking to provide an account of the history of science, it is important to ensure that we identify a general logical structure that is adequate to describe the entire sequence of theories. In other words, where Reichenbach and Friedman seek to provide the conditions for the possibility of a given scientific theory, Cassirer sought to provide the conditions for the possibility of the sequence of scientific theories within a given field.

The distinction between the two approaches is even more marked in their attitudes towards Kant's division of the human intellect into sensibility and understanding. Friedman seeks to maintain some sense in which the manifold of intuition can be retained in contemporary philosophy. Cassirer's historicised transcendental logic provided a manner in which to pursue a Kantian philosophy without the manifold of intuition. The key to seeing how this is possible is Kant's Schematism:[128] here, recall, Kant argued that objects could be brought under concepts only by appeal to mediating—spatial and temporal—*schema*. Without the manifold of intuition, this solution is impossible. As such, Cassirer's transcendental logic sought to provide an alternative to the Schematism according to which objects are brought under concepts by emphasising a functional understanding of objects. So, Cassirer's argument that the concept <object> is constituted by the concept <objectivity> was his means to provide an alternative to Kant's Schematism.

The details of how objects are constituted differs according to the type of object—for the objects of natural science the relevant fundamental epistemological concepts are understood in the following fashion:[129]

[128] See §2.2.2.

[129] See (Heis, 2011a) for an account of how precisely the Schematism is carried out for mathematical objects.

(a) <Object>: This concept refers, in the first instance, to the individuals whose behaviour is described or predicted by a scientific theory. These individuals can be either observable or unobservable. Examples of instances of <object> are solid mass particles (in classical physics), space-time in the theory of relativity[130] and electrons in quantum physics.

(b) <Truth>: Hertz, as we saw in the previous section, emphasised the role of models within a scientific theory. Models, and their success in representing reality, provide a helpful way to understand the concept <truth> in science. <Truth> applies to representations: it is a question of whether the empirical laws that hold between individuals in the models of a theory correspond to what is observed in experience. It is purely about correspondence between the relations of a model and phenomenal relations. Examples of instances of <truth> are Newton's law of gravitation, Einstein's field equation and the Schrödinger equation.

(c) <Knowledge>: Like <truth>, <knowledge> is best understood in terms of representation. <Knowledge>, though, requires more than <truth>: in particular, representations that count as <knowledge> must, in addition to accurately recreating the phenomenal relations between bodies, provide an additional, non-observable basis for these relations. Examples, then, of representations that constitute instances of <knowledge> are those that capture Newton's laws of motion, the equivalence principle and the Pauli Exclusion Principle.

(d) <Objectivity>: This concept is intended to capture the idea that science should not tell us just about a single observer and his sense impressions. A theory's objectivity consists in the fact that its results are independent of any preconditions that are unique to a given observer.[131] <Objectivity> is now commonly expressed in terms of invariance principles: these, e.g., ensure that whatever an observer's spatio-temporal location may be the laws of physics take the same form. Examples of instances of <objectivity> are Galilean

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[130] As we will see in the next chapter, one of the interesting features of the development of relativity theory relates to how space-time moves under the concept <object> from the concept <knowledge>.

[131] It is important to stress that, for Kant, a theory's objectivity consists in its being built upon preconditions of knowledge that are common to all (human) observers: i.e. the categories of the understanding.

invariance, Lorentz invariance, general covariance[132] and permutation invariance.[133]

On the substance-theory of concepts then, the concept <object> is fundamental and understood as referring to physically real individuals. Let us see how Newtonian physics would be interpreted on the substance-theory. So, first the truth of Newton's law of gravitation would be understood in terms of the concept <object>. That is, a model describing the force experienced on a falling object on earth is true if it accurately describes the behaviour of a physical object free-falling to earth. <Knowledge> would, then, be explained in terms of the concept <truth>. So, in Newtonian physics, an instance of <knowledge> would, e.g., be the representation of a solitary mass particle with no force acting on it. There is no way to justify the claim that this representation corresponds to experience by straightforward observation: this represents a situation with no physical analogue. Instead one must argue that, given the observed regularities among objects, i.e. <truth>, this representation does correspond to what *would be* observed if it was possible to observe a solitary, force-free object. Finally, <objectivity> is explained in terms of <knowledge>: those representations that count as <knowledge> govern the behaviour of objects in the world and, as such, the theory is objective in the sense that it gives predictions about objects that are confirmed by any observer.

In the case of mathematics, I think it is clear that Cassirer understood the function-theory of concepts to involve the claim that the concept <object> should be analysed in terms of <objectivity>.[134] However, the function-theory of concepts did not apply as straightforwardly to natural science as it did to mathematics. In 1910, when Cassirer wrote SF, he was most familiar with the work of Helmholtz and Hertz and took their work to represent the most significant recent developments in how natural scientists understood

132 Where this is understood as the invariance of the form of physical laws under arbitrary (active) coordinate transformations.

133 That Cassirer understood the conceptual categories in this fashion is suggested in DIMP. Here Cassirer argued that there are three types of statement in science: statements of the results of measurements (ch.3), statements of law (ch.4) and statements of principles (ch.5). These, I suggest, correspond to three of the four fundamental epistemological concepts that Cassirer discussed in SF: i.e. results of measurements are instances of the concept <object>, laws are instances of the concept <truth> and principles are instances of the concept <knowledge>.

134 Cassirer, e.g., is quite clear that it is only meaningful to hold a system of relations as fixed and constitutive of the concept <number> when we hold "the form of the system itself as an *invariant*" (SF, p.40). That is, it is the objectivity of the system of relations that defines the concept <number> that is ultimately what explains the possibility of the structuralist account of numbers. Heis (2011b) makes a detailed case for this being how Cassirer understood the function-theory of concepts in mathematics. I am in complete agreement with Heis's account here and so, rather than rehearse the details of how the function-theory of concepts applies to mathematics, I will instead focus on Cassirer's more problematic discussion of the sense in which natural science has adopted the function-theory of concept formation.

the nature of concepts.[135] However, both Helmholtz and Hertz took the concept <object> to be the most fundamental explanatorily concept. I argue that the physical concepts that Cassirer surveyed in SF, then, do not support the claim that the natural science of the time had embraced the function-theory of concepts in the same sense as mathematics had.[136] It is only when Cassirer had general relativity and quantum physics available that he was able to clearly demonstrate that natural science, like mathematics, had transitioned to the function-theory of concepts.

In SF the most recent developments in natural science that Cassirer discussed were Helmholtz's theory of signs and Hertz's model of classical mechanics. Hertz, as we saw in §2.2, sought to explain all the phenomena of classical mechanics in terms of just the three fundamental concepts of space, time and mass. Cassirer focussed his discussion of Hertz's model of mechanics on the means by which mathematical symbols came to represent "concrete sensations" (SF, p.184).

Hertz had divided his (2007) into two parts: the first part considered space and time as "a priori judgments in Kant's sense" (p.45), that is, as they are offered to "inner intuition". He intended to develop the mathematics of his account in a manner that was independent of how the key concepts that he appealed to in his model were actually used in experience. It was only in the second part of the *Principles of Mechanics* that Hertz sought to connect his mathematical system to the geometry and kinematics of the physical world. This was achieved by taking certain fixed units of measurement as a basis and using these to compare empirical spaces, times and masses.[137] So, to take the simplest example, in the first part of his (2007), Hertz had defined the position of a particle as being "the point of space which is indicated by a given particle at a given time" (p.48). How should this feature of the model of mechanics be understood to correspond to external experience? Instead of being determined with respect to space understood as an absolute, given by inner intuition,

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135 That Cassirer took Helmholtz and Hertz to be the key figures in the development of scientific epistemology since Newton is clear from the introduction to ETR. Here, Cassirer cites both Helmholtz and Hertz as the scientists between Newton and Einstein who contributed to developing answers to epistemological questions in physics (ETR, p.354). Furthermore in SF, Cassirer cited Hertz as being responsible for having "finally developed" a new understanding of the concepts of space and time (SF, p.170). He also discussed Helmholtz's theory of signs and the impact that this theory had on his understanding of objects (SF, p.304ff.).

136 This, I suggest, is what Cassirer had in mind when he wrote in DIMP: "I have attempted elsewhere [SF] to show at length how this "substantialistic" conception underwent a gradual change…In these considerations I confined myself to the development of classical physics and its contemporary situation, but they could have been formulated much more briefly and precisely had I had the general relativity theory and modern atomic physics before me at the time" (p.131). As we will see, the conceptual advancements of Helmholtz and Hertz only secured the first of the two features of the function-theory: i.e., they suggested that we had knowledge of objects only through their relations. It is only in ETR and DIMP that Cassirer clearly explains how the second feature of the function-theory is suggested by the development of natural science.

137 "We mean to say that by our senses we can determine no time more exactly than can be measured with the help of the best chronometer, no position more exactly than can be referred to the coordinate system of the remote fixed stars, and no mass more exactly than is done by the best scales." (2007, p.141)

position is to be determined with respect to the coordinate system that is defined by the fixed stars.[138] Similarly, Hertz argues that the ideas of space and time that feature in his model correspond to measurements with rigid bodies and clocks respectively.

Cassirer (SF, p.185) pointed out that even if, for some reason, we have to abandon the fixed stars as a reference frame for mechanics, we can equally well select another and doing so in no way affects the meaning of Hertz's model: "its empirical realization would only be shifted to another place" (*ibid.*). Cassirer suggested that this meant that the absolute space of mechanics, rather than being something that is given, is something that is always sought. So, for example, if we can no longer take the fixed stars to serve as a reasonable approximation of the absolute space of inner intuition, we would go beyond the fixed stars to find something else that would serve as a reasonable empirical realisation of absolute space. So, Cassirer, in a similar way to Kant on Friedman's reading,[139] interpreted absolute space as a regulative ideal: that is, the end-point of a series of frames of reference that permit us to coordinate the space that appears in our models of mechanics with empirical space. Cassirer understood this as meaning that "the physical space of bodies is no isolated essence but is only possible by virtue of the geometrical space of lines and distances" (SF, p.186): the physical concept of space is constructed, aiming at the regulative ideal of absolute space, from relationships between bodies and this, in turn, can only be known through measurements of distances. This is the sense in which Cassirer took Hertz's science to support the function-theory of concepts in SF: Hertz's mechanics was based upon the idea that the physical concept of space was coordinated to the ideal of absolute space by relationships between bodies.

It is clear, then, that Cassirer understood the development of science to have shown that the concept of space should be understood relationally. However, there is no mention of the second part of the function-theory of concepts, according to which <objectivity> is the fundamental explanatory concept. From our discussion of Hertz's philosophy in §3.2.2, it is clear why Cassirer did not yet have the resources to make this additional claim. Hertz's *Bildtheorie* was conceived as a means to solve Helmholtz's problem of validity: it provided a means, beyond trust in causal regularity, to think that our physical models might represent reality. Hertz assumed, that is, that the objects and concepts that were found in his model of classical mechanics—space, time and bodies—literally referred to objects and concepts with counterparts in the physical world.

There is a clear contrast between Cassirer's account of classical mechanics in SF and his account in ETR. It was general relativity that made it clear to Cassirer that the

[138] See (Hertz, 2007, p.140).
[139] See §1.3.2.

function-theory of concepts must be applied to the realm of natural science in precisely the same fashion as it had been in mathematics. General relativity, on Cassirer's reading, clearly explained the possibility of the concept <object> in terms of the concept <objectivity>. This is why the first question Cassirer asks about general relativity is the following:

> Each answer, which physics imparts concerning the character and the peculiar nature of its fundamental concepts, assumes inevitably for epistemology the form of a question. When, for example, Einstein gives as the essential result of his theory that by it "the last remainder of physical objectivity" is taken from space and time ([Einstein, 1916, p.153]), this answer of the physicist contains for the epistemologist the precise formulation of his real problem. What are we to understand by the physical objectivity, which is here denied to the concepts of space and time? (ETR, p.356)

The "essential result" of the theory of relativity, according to Einstein, is seemingly the denial that objectivity about space and time is possible. No wonder, then, that Cassirer took this to be the "real problem" for the epistemologist: Einstein seemed to be claiming that the most fundamental concept in Cassirer's understanding of epistemology is be incompatible with physics. This is how Cassirer begins ETR and, it is clear, that one important function of ETR is to argue that Einstein's understanding of "physical objectivity" differs from the epistemologist's understanding of the concept <objectivity>.[140]

Cassirer is quick to rule out two of the more obvious potential answers to this question. First, he considers Planck's formulation of a criterion for physical objectivity "that everything *that can be measured* exists" (ETR, p.357). This, Cassirer says, cannot help the epistemologist: as soon as an epistemologist considers even the simplest measurement it is clear that it must rest upon various theoretical propositions.[141] It is also insufficient to try and suggest that Einstein is simply denying that space and time possess physical objectivity in the sense that naïve realists mean. In light of Cassirer's analysis of concepts in

[140] My reading here is in contrast to Ihmig's who introduces this passage as follows: "Even at the beginning of his deliberations in *Zur Einsteinschen Relatiditvtstheorie,* he points explicitly to a comment by Einstein on the problem with the concept of the object" (1999, p.524). Ihmig takes the central question that the passage raises to be: "How relevant is the principle of the logical priority of the concept of law over the concept of object for the special and the general theory of relativity?" (*ibid.*). As we have seen Cassirer takes the concept <object> and the concept <objectivity> to be quite different: as such, given that Cassirer is talking here about the concept <objectivity> then, I think, it must be mistaken to view Cassirer as being concerned in this passage with the concept <object>. The problem is not that the space and time of general relativity are not objects because, in Cassirer's sense, they are (in that the object space-time features in representations of the world). The problem is, instead, that relativity seems to undermine the idea that space and time are objective: after all, no particular representation of space is diffeomorphism invariant. This, I suggest, is why it is important to emphasise that there were two aspects to Cassirer's understanding of the function-theory of concepts.

[141] Cassirer takes this to have been clearly demonstrated by Duhem's analysis of the physical construction of concepts (SF, p.138).

SF, though, this is true of any concept: ‘the property of not being thing-concepts but pure concepts of measurement, space and time share with all genuine physical concepts’ (*ibid.*). In dealing with space and time we must recognise that they have a special logical position; they are forms of measurement “a step further removed” than the likes of energy and mass. If one is to successfully analyse Einstein’s remark then one must be sure to analyse the concepts of space and time within their special logical position.

Cassirer—in keeping with Marburg idea that transcendental philosophy begins with the fact of science—intended to show that the question as to what it means for general relativity to deprive space and time of their physical objectivity must be answered by examining the development of the concept of objectivity within physics. This is how Cassirer’s argument proceeds (ETR, pp.356-81) until he eventually reaches the conclusion that classical mechanics had “believed itself at the goal” of physical objectivity too soon: “It clung to certain reference bodies and believed that it possessed, in connection with them, measures in some way definitive and universal, and thus absolutely ‘objective’” (p.381). Cassirer, then, understood Einstein as claiming that the classical understanding of objectivity was in error. The problem was that classical mechanics began with “reference bodies”—i.e., the concept <object>—and took these to represent something universal from which an “objective” understanding of the universe could be constructed. In relativity, objectivity means something quite different:

> For the new theory, on the contrary, true objectivity never lies in empirical determinations, but only in the manner and way, in the function, of determination itself…To wish to know the laws of nature without relation to any system of reference is an impossible and self-contradictory desire; all that can be demanded is that the content of these laws not be dependent on the individuality of the system of reference...Measurements in one system, or even in an unlimited number of "justified" systems would in the end give only particularities, but not the true "synthetic unity" of the object. The theory of relativity teaches, first in the equations of the Lorentz-transformation and then in the more far-reaching substitution formulae of the general theory, how we may go from each of these particularities to a definite whole, to a totality of invariant determinations. (*ibid.*)

Cassirer is quite clear here that objectivity should be understood as meaning only that the laws of physics should take the same form in any frame of reference.[142] In pointing out that even measurements in an unlimited number of “justified systems” will only give a series of particularities, Cassirer explained why his account of Hertz’s mechanics in SF does not

[142] In general relativity this amounts to the claim that what is objective is that which is diffeomorphism invariant. I return to this in §3.3.2.2.

quite amount to embodying the full function-theory of concepts: Hertz's methodology would lead to us always viewing the laws of nature from one particular perspective. To avoid this, one must start from the idea of objectivity—as embodied in the invariance principles of the theory of relativity—and it is this that will ultimately secure the "synthetic unity" of physical concepts such as space. By ETR, then, it is quite clear that Cassirer understood the fundamental explanatory concept of physics to be <objectivity> and that he understood this concept to be instantiated by invariance principles.[143]

It is also clear in DIMP that Cassirer thought that the concept <object> could not be fundamental.[144] In the early part of DIMP Cassirer says little about how the relationship between principles, laws and measurement results (corresponding to the concepts <knowledge>, <truth> and <object>). There is just a small clue in his discussion of statements of the results of measurements:

> If we choose a spatial analogy for the structure of physics, we must not liken the structure to a pyramid resting on a broad base of immediately given and independent "facts," rising gradually from this and ending in a highest point…For this would involve overlooking the mutual interconnection and forgetting that "everything significantly factual is already theory." There would always be the possibility of imagining the highest layers removed without destroying the bottom layer or even altering it essentially. But we will attempt to show in detail later why such an assumption is untenable and impracticable for every part of physical knowledge. Physics accordingly is to be compared not to a pyramid but…to the "well-rounded sphere". (DIMP, p.35).

The understanding of physics as a "pyramid-structure" corresponds to the substance-theory of concepts. At base there are the results of series of measurements—which, as we have seen, is just how Cassirer understood modern physics as describing objects—and from this we would derive laws, which are explained in more depth by appeal to principles and which altogether finally gives an objective view of reality. Cassirer is very clear that this is not how he understands the three different types of physical statement as functioning. This is because, in a pyramid it is possible to remove higher levels without altering the lower level. However, in the case of physics if we change either the statements of principles or the statements of laws, we affect the statements of the results of experiments. Cassirer does not explain why this is the case at this stage of DIMP: in this part of the book he is

143 Cassirer's discussion of Helmholtz in SF is similar to his discussion of Hertz in the respect that he does not yet take Helmholtz to advocate the full version of the function-theory of concepts. Cassirer described Helmholtz as advocating a relational account of objects, even though he still retained the concept of <object> as fundamental (SF, p.304ff.).

144 Where Cassirer understood the concept <object> in terms of the results of experiments (DIMP, p.36).

simply intending to draw attention to the fact that there are three different classes of statement because he thought this had been overlooked.[145]

Cassirer eventually resolved this problem in terms of substance-concepts and function-concepts and it is here that he most clearly associated Helmholtz and Hertz with the substance-theory of concepts. This indicates a slight shift in Cassirer's understanding of the substance-theory of concepts between ETR and DIMP. In SF Helmholtz and Hertz were both only associated with the function-theory of concepts because, in the case of Helmholtz he had argued that objects could only be known via relations and in the case of Hertz because he advocated a relational understanding of space. In his discussion of Helmholtz, we saw Cassirer indicate that Helmholtz still had an idea of "absolute objects": however in SF Cassirer focussed on the fact that even with an idea of absolute objects, Helmholtz still argued that they could only be known through their relations.[146] The move towards the function-theory of concepts proper was only possible in natural science after the development of general relativity.

In DIMP, Cassirer clearly indicates that he considers both Helmholtz and Hertz to have advocated the substance-theory of concepts on the grounds that they took matter to be the "fundamental reality" which scientific concepts seek to describe. Cassirer argued that Helmholtz's central claim—that sense impressions are subjective signs of external objects and that the interpretation of these signs is the work of the intellect—sought to explain objectivity in terms of persisting external objects:

> The objectively real is presupposed as something persisting and substantial, but in its substantial existence it cannot fit into the sign language of our concepts of nature. This language is merely able to reproduce relations between individual phenomena but not the general substratum underlying them. (DIMP, p.130)

Cassirer, then, understood Helmholtz to have taken a step towards the function-theory of concepts insofar as he argued that the language of the sign-theory could only hope to capture relations between phenomena, while having no access to the underlying reality. This means, Cassirer argued, that "the substantialism essential to the mechanical viewpoint always contains a trait of agnosticism: the unknowable becomes a presupposition of knowledge itself" (*ibid*.). This is how Cassirer understood the influence of Helmholtz's work: in taking the senses to be a mediating realm between the human intellect and external reality we must always maintain a degree of agnosticism about whether science provides knowledge of external reality. It is this idea that gave rise to the problem of validity: i.e.

145 This goal is explicit, see (DIMP, p.30).
146 See (SF, pp.288-9).

given the subjectivity of our representations of the world, how can we have objective knowledge? Cassirer, by taking objectivity as the foundational epistemological concept, was able to side-step this problem: the concept <object> is constituted by the concept <objectivity>. There is, thus, no problem of how our knowledge of objects is objective: we are only able to know objects that are embedded in a structure defined by specific invariance principles.

Thus, in the final analysis, we should understand Cassirer's fundamental distinction between the substance-theory and function-theory of concepts as follows. The substance theory posits that there are externally existing objects that ultimately explain the success of science. That is, it begins with the concept <object> and seeks to explain the concept <objectivity> via <truth> and then <knowledge>. The function-theory begins from the fact that scientific knowledge is objective and seeks to explain the possibility of objects in terms of objectivity. That is, the order of the epistemological concepts is reversed and the concept <object> is accounted for ultimately in terms of <objectivity> via the concepts of <knowledge> and then <truth>. This is the most significant meaning of the function-theory of concepts. With this understanding of the function-theory of concepts in place, we are in a position to detail Cassirer's answers to CR and CC. This will be the task of the next section.

3.3.2. *Cassirer's answers to the two challenges: regulative principles, the a priori and the rationality of science*

In the previous section I have sought to clarify Cassirer's function-theory of concepts and show how this constitutes a reasonable modification of Kant's insights in light of the development of modern science and mathematics. Cassirer was Kantian in the sense that he sought to historicise Kant's transcendental logic: this involved (i) inquiring as to the objectivity of the entire sequence of scientific theories and (ii) taking the function-theory of concepts to stand in for Kant's Schematism. We are now in a position to ask whether Cassirer's regulative Kantianism is able to provide satisfactory answers to CR and CC. In §3.3.2.1 I detail Cassirer's answer to CR and in §3.3.2.2 I address CC and argue that a sense of constitutivity can be retained even within a regulative framework.

3.3.2.1. *Regulative principles and the rationality of science*

The idea that the entire sequence of scientific theories is objective is central to Cassirer's methodology. His account of the a priori was intended precisely to secure the objectivity of science as a whole. The reason that Cassirer needed to appeal to something beyond experience in order to secure the objectivity of science should be clear from the discussion of the previous section. A key claim in his analysis of the history of science was that statements of the results of measurements depend upon statements of laws and statements of principles. This is equivalent to the well-known claim that experiment is theory-dependent. Now, from our present perspective, this means that measurements do not necessarily mean the same thing before a scientific revolution as they do after a scientific revolution: as such, appeal to the results of experiments cannot be sufficient to demonstrate that, e.g., Einstein's theory of relativity has the same subject matter as Newton's theory of gravitation.[147] The version of CR that Cassirer's Kantianism had to face, then, contains the version of CR that Friedman seeks to address. However, Cassirer has an additional problem to face. For Friedman, the challenge of rationality arises because there is a real sense in which precursor and successor theories refer to different worlds: constitutive principles define a theory's logical space, so, from the perspective of a precursor theory, the claims of its successor will not even represent a coherent possibility. CR, for Friedman, consists in explaining how it can be rational for scientists to adopt a new framework when the phenomena described by that theory are not even possibilities for those scientists. Cassirer had also to show that the worlds described before and after theory-change are the same, in spite of the fact that even statements of the results of measurements need not mean the same thing after a revolution as they did before.[148] Of course, even with this difference, Cassirer still also had to explain the rationality of scientific theory change in the more familiar manner that we discussed in the first chapter. This means that CR, for Cassirer, can be stated as follows:

CR: (i) How is it possible to treat the sequence of past scientific theories as part of a single logical structure?
(ii) How can it be rational to abandon an established conceptual framework in favour of a new one?

[147] In SF Cassirer gives numerous examples to show that measurements presuppose various other concepts. E.g. he argued that the temperature measured by a thermometer depends on a "network of theoretical presuppositions" about geometry and the expansion of mercury (SF, p.141ff); he argued that Regnault's test of Mariotte's law—which states that, at constant temperature, the pressure of an ideal gas is inversely proportional to the volume—depends on assumptions about the apparatus used to establish the volume of the gas related to geometry and general and celestial mechanics (p.143ff); he argued that measurement is not possible without first identifying units to treat as constants, but this constancy "is never a property that belongs to the perceptible as such, but is first conferred upon the latter on the basis of intellectual postulates and definitions" (p.144ff).
[148] See (SF, pp.143-6).

Cassirer sought to address (i) by identifying those elements that remain invariant throughout all experience. These "invariants of experience" were introduced in the context of the discussion of induction with which he begins the second part of SF. Cassirer understood induction as involving one of two processes: "the gaining of particular facts and the connecting of these facts into laws" (p.265). In both of these cases inductive thought is characterised by the tendency to search for "something permanent in the coming and going of sensuous phenomena" (p.249). He explains how the development of geometry displays precisely this tendency:

> Thus all experience is directed on gaining certain "invariant" relations, and first in these reaches its real conclusion. The conception of the empirical natural object originates and is grounded in this procedure; for it belongs to the concept of this object that it remains "identical with itself" in the flow of time. We must, indeed conceive each natural object as subject in principle to certain *physical* changes, called forth by external forces; but the reaction to these forces could not be represented in the form of law, if we were not able to recognise the object as logically permanent and provided with the same properties. (p.250)

This, I take it, is what Cassirer meant when he claims that "in both cases [of induction], the problem is to raise from the flux of experience that which can be used as *constants* of theoretical construction" (p.265): the development of mathematics, geometry and science is marked by an attempt to isolate, from amongst all the changing features of experience, those connections that are permanent and can be retained. In the case of geometry we see this in the move towards group-theoretic accounts, whereby the various types of geometry are coordinated to a definite group of transformations.

Central to the scientific method is the second moment of induction that Cassirer mentions: the connecting of empirical facts into laws. Empirical laws, for Cassirer, are "constants of a higher order" that are arrived at by isolating and superposing simpler relations. So, empirical laws are just the result of the human tendency to seek something unchanging in experience. Now, as we have seen, Cassirer takes the history of scientific development to be the proper subject of transcendental inquiry, so his account of empirical laws cannot stop here. Instead, he argues that the constants that are found in earlier sciences become variables for later sciences and the demand to seek constants is the applied to these new variables.[149]

[149] "Thus we stand before a ceaseless progress, in which the fixed form of being and process that we believed we had gained, seems to escape us. All scientific thought is dominated by the demand for unchanging elements, while on the other hand, the empirically given constantly renders this demand fruitless. We grasp permanent being only to lose it again. From this standpoint, what we call science appears not as an

The demand of science, then, is to seek elements that are invariant throughout all scientific development. As such, science will reach its end point when there are no more variables. Cassirer, then, posited certain ultimate invariant concepts that serve as a regulative ideal and govern the development of science.[150] This regulative ideal is one way—though, as we will see, there are others—in which Cassirer understood the a priori:

> From this point of view, the strictly limited meaning of the "a priori" is clearly evident. Only those ultimate logical invariants can be called a priori, which lie at the basis of any determination of a connection according to natural law. A cognition is called a priori not in any sense as if it were prior to experience, but because and in so far as it is contained as a necessary premise in every valid judgment concerning facts. If we analyse such a judgment, we find, along with the immediate contents of sensuous data and elements differing from case to case, something permanent; we find, as it were, a system of "arguments," of which the assertion involved represents an appropriate functional value. (SF, p.269)

So, this meaning of the a priori extends to that which is contained in "every valid judgment concerning facts". The ultimate goal of science—the ideal limit towards which the scientific endeavour is aimed—is to identify those elements of experience that remain unchanged from theory to theory.[151]

Friedman interprets this as meaning that, for Cassirer, the a priori is constitutive of the entire sequence of scientific knowledge.[152] We need, though, to be very careful about how we understand the constitutive role of this regulative ideal. Cassirer does clearly assign some constitutive role to the ultimate invariants of experience, claiming that they

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approximation to any "abiding and permanent" reality, but only as a continually renewed illusion." (Cassirer, SF, p.266)

150 Examples of the ultimate invariants of experience might be concepts such as <space>, <time>, and <cause>. These are all concepts that Cassirer thinks will continue to underwrite all scientific knowledge. However, he is clear that he does not necessarily expect them to be interpreted in the same way throughout science. For example, Cassirer argued that the development of the theory of relativity has replaced the concept <space> as it was understood in classical physics with a concept of <spatiality-in-general>, which is just the claim that objects must stand in some form of spatial relation. Clearly the actual invariants of experience cannot be known until we reach the end-point of science, but from the present perspective these concepts—understood in the most general possible terms—serve as an approximation of what sort of concepts might be part of the regulative ideal.

151 As Ferrari (2012) stresses, this represents an extension of Marburg neo-Kantianism. For Cohen, the importance of the a priori lay in making experience—specifically scientific experience—possible. Given the rejection of the distinction between sensibility and understanding, Cohen's transcendental philosophy must have a quite different direction to Kant's. For Cohen, the goal of transcendental philosophy is to 'anticipate a priori the general form of experience according to the "plan of reason"' (*ibid.*). Cohen's vision of transcendental philosophy, then, suggests that we start from the historically given fact of natural science and seek to uncover the a priori foundations of scientific facts. SF represents a clarification of this aspect of Cohen's philosophy: we can attempt to uncover the a priori foundations of science by seeking those concepts that are invariant throughout all changes in the material content of scientific theories.

152 See Friedman (2000, p.111; 2010, p.783n273). He reads Cassirer as arguing that if the regulative ideal is ever attained, then the ultimate invariant concepts will be understood to have been constitutive of all scientific theories even though this is only clear with hindsight.

"constitute both the presupposition and goal of investigation" and that "the system of these unchanging elements constitutes the type of objectivity in general,—in so far as this term is purely limited to a meaning wholly comprehensible to knowledge" (SF, p.277). What does Cassirer mean here?

The situation is clarified if we distinguish between two notions of objectivity in Cassirer's work: "objectivity in general" and "objectivity at any time". The distinction is just that "objectivity in general" refers to the ultimate invariants of experience, while "objectivity at any time" just refers to the particular invariance group of a given theory. The primary role of the ultimate invariants of experience in Cassirer's philosophy is to answer the first part of CR, by providing an account of the unity of successive scientific theories. Whereas the scientific realist locates the unity of science in physical reality by arguing that successive theories give successively better approximations of physical reality, this is not an argument that is open to Cassirer. Cassirer, instead, located the unity of science in the ultimate invariants of experience. That is, Newtonian physics and the theory of relativity are "about the same subject" in the sense that limited versions of the ultimate invariants of experience feature in the construction of both theories. So, for example, the idea of space as a three-dimensional container for objects whose structure was described by Euclidean geometry was central to the construction of Newton's theory of gravitation. In the theory of relativity, this understanding of space is replaced by the idea that space has a dynamic structure that is dependent upon the presence of masses and, crucially, the idea of space as an object is replaced by the understanding of space in terms of "pure relations of measurement" (ETR, p.446). So, "objectivity in general" is grounded by the idea that the ultimate invariants of experience are captured to an increasing degree of approximation as science develops.[153]

The central claim of the Marburg School was that the object of scientific knowledge is not "given [*gegeben*]", but is, instead "set as a task [*aufgegeben*]". Objectivity in general, then, is precisely that which is "set as a task" for Cassirer. It is a crucial component

[153] Ryckman's (1999; 2005) discussion of Cassirer's account of general relativity places great emphasis on Cassirer's talk of de-anthropomorphisation. This, I think, is helpful; in understanding the role that the ultimate invariants of experience play. Cassirer (SF, pp.304-8) suggested that one of the fundamental results of Helmholtz's philosophy is that we can only know of the properties of objects through their relation to our senses—this is just a result of the theory of signs that was introduced in §2.1. It may be helpful, then, to consider the ultimate invariants of experience as how we would eventually understand fundamental concepts such as <space>, <time> and <cause> once we have removed all remnants of sensibility from them. If the problem of objectivity is understood as being the problem of how the statements of experimental results in science are the same for all observers, even though the results must be mediated by each individual's senses, then we can see why Cassirer would treat the ultimate invariants of experience as the end-point of a process of de-anthropomorphisation. By removing any trace of sensibility from these concepts, we have removed the feature of experience that placed the objectivity of experience in doubt in the first place.

of answering CR as it applies to Cassirer's work because it ensures the unity of the subject of scientific study.

There is, of course, a rather striking problem with seeking to secure the unity of the subject of scientific study in this manner: we will only discover the ultimate invariants of experience at the ideal end-point of the scientific process, when we will be able to look back at the history of scientific theories and see the extent to which each of these theories instantiated the ultimate invariants. This is precisely Friedman's objection to the regulative approach: he objects that the a priori elements that are constitutive of experience are never known in advance and will only be known at the end-point of scientific inquiry. For Friedman, this means that the *prospective* rationality of theory change cannot be secured on Cassirer's account; it is only possible to explain the retrospective rationality of adopting a new physical theory.[154] We can, however, begin to develop a response to this problem on Cassirer's behalf.

Cassirer did not just appeal to the idea of "objectivity in general"; he also appealed to a notion of "objectivity at any time":

> [We] can very well replace a relatively narrower aspect of experience by a broader, so that the given data are thereby ordered under a new, more general point of view. The earlier results are not thereby rendered valueless, but are rather confirmed within a definite sphere of validity. Each later member of the series is necessarily connected with the earlier ones, in so far as it answers a question latent in them. We face here a perpetually self-renewing process with only relative stopping-points, and it is these stopping points, which define the concept of "objectivity" at any time. (SF, p.278)

So, for Cassirer, as science advances it replaces a "narrower aspect of experience by a broader" aspect of experience. Here Cassirer is best read as referring to science developing by broadening its invariance group—e.g., when he was writing SF, the Galilean invariance of classical mechanics had been broadened to the Lorentz invariance of special relativity.[155] Science, as we have seen, is understood as a sequence of theories making successively better approximations of the ultimate invariants of experience: this is the "perpetually self-renewing process with only relative stopping-points". The stopping-points, where a theory satisfies one particular invariance group, then define the concept <objectivity-at-any-time>.[156]

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154 See (Friedman, 2010a, p.783n273).

155 Lorentz invariance is broader than Galilean invariance in the sense that it includes Galilean invariance as a limiting group for low velocities.

156 Cassirer's use of scare-quotes around objectivity in the above quote suggests that he does not understand this to be a true notion of objectivity. Indeed, Cassirer could not possibly consider objectivity-at-any-time as indicative of objectivity-proper. He is quite clear in SF that "There is no objectivity outside of the frame of

That this is what Cassirer had in mind is clear from his discussions of both Minkowski's version of special relativity and Einstein's general theory of relativity. Minkowski's theory of relativity sought to derive Einstein's relativistic results by using group-theoretic mathematical methods: he reformulated the principle of relativity in terms of the four-dimensional Lorentz invariance group. This invariance group, then, formed the starting point of Minkowski's analysis of special relativity.[157] In ETR (pp.424-5), Cassirer went through the implications of Minkowski's theory of relativity and argued that Minkowski's treatment of the transformation equations as an invariance group served to secure a sense of objectivity:

> [Here] too the transformation-equation reëstablishes objectivity and unity, since it permits us to translate again the results found in one system into those of the other. (ETR, p.425)

There is a distinction, then, to be drawn between the ultimate invariants of experience and invariance principles on Cassirer's account. The ultimate invariants of experience are those elements of our knowledge that, when we have reached the end-point of science and retrospectively consider the series of scientific theories, are presupposed in each theory. From the development of space-time theories, Cassirer suggested that <spatiality-in general>, <temporality-in-general> and <magnitude> may, eventually, be understood as ultimate invariants of experience. This is quite different from the role that invariance principles play. The goal of the scientific endeavour is to seek out the ultimate invariants of experience; but how should this goal be pursued?

Cassirer answers this by examining the development of science. Science, it is clear, has not advanced by hoping to project the ultimate invariants of experience and incorporate these into its theories. Such a task would, of course, be hopeless for Cassirer since the ultimate invariants of experience can only emerge in retrospect. Instead scientific theories have progressed by widening their invariance groups: the Galilean invariance of Newtonian physics was widened to Lorentz invariance in the development of special relativity and this was widened again to general covariance in general relativity. At each stage of the development of the sequence of scientific theories, there is an invariance group that establishes what is objective according to the theory—in general relativity for instance, objectivity is determined by diffeomorphism invariance—and the invariance groups become broader as theories approach the ideal limit. This is why Cassirer repeatedly

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number and magnitude, permanence and change, causality and interaction: all these determinations are only the ultimate invariants of experience itself, and thus of all reality, that can be established in it and by it" (p.309).

[157] See (Minkowski, 1915).

mentions the importance of de-anthropomorphisation for the progress of science.[158] The invariance principles provide a sense of objectivity in that they act as a means to escape the parochial perspective of individual observers. Invariance principles, then, act as approximations of the final, general concept of <objectivity> that act as the fundamental explanatory concept at each stage of the development of science.

We now have the answer to the first part of Cassirer's version of CR, and the beginnings of an answer to the second part. The history of science is treated as all part of a single logical structure with the common goal of revealing the ultimate invariants of experience. Furthermore, we have seen that at each stage of science there is a concept of <objectivity> embodied by the invariance group of the theory that serves as the explanatorily fundamental concept at that period of the development of science. So, to provide an account of the rationality of adopting a new framework, we must ask what makes it rational to adopt a new invariance group.

This is the feature of scientific development that Friedman argues that Cassirer's account is ill-suited to capture. Friedman takes Cassirer to have made just one demand on the development of scientific theories: that each stage should "continuously emerge" from the previous state. For Friedman, this methodology "sheds no transcendental light on the actual historical process by which we arrived at general relativity in the first place" (2010, p.783n.273). That is, while Cassirer provided an explanation of why current scientific knowledge is rational with respect to the ideal limit, he could not explain the rationality of the process by which a new theory emerges from an old one.[159]

This, as should now be clear, cannot be the only aspect of Cassirer's account of the rationality of theory change. The ultimate invariants of experience serve to ensure that science, throughout its history, is addressing the same subject matter. They are used to retrospectively identify a priori elements throughout the history of science. However, these a priori elements should not be understood as constituting either individual theories or the historical sequence of theories. These a priori elements are not constitutive of individual theories simply because they are not sufficient to make possible the empirical side of the theory. For instance, the ultimate invariant of experience <spatiality-in-general> is not sufficient to derive Newton's law of gravitation: it must also be treated as three-dimensional and described by Euclidean geometry. The ultimate invariants of experience should also not be constitutive of the entire sequence of theories. Identifying elements that are approximated in each of a sequence of theories does not make the sequence of theories possible; it merely ensures that the sequence has a single subject matter.

158 See (SF, p.306), (ETR, pp.381-2, p.421, p.445)

159 Friedman stresses this point again in his (2010b, p.186).

Cassirer, in both SF and ETR, provided a clear account of what would make the abandonment of an old theory for a new one rational. He discussed the possibility of abandoning the principles upon which Newton's dynamics was constructed, arguing that they do not need to be taken as unchangeable: "they can rather be regarded as the temporarily simplest intellectual hypotheses by which we establish the unity of experience" (SF, p.268). He also provided an account of what it would take to justify the abandonment of these principles. First, Cassirer clarified that there are situations in which we would accept changes to the principles without abandoning them: "We do not relinquish the content of these hypotheses, as long as any less sweeping variation, concerning a *deduced* element, can re-establish the harmony between theory and experience" (*ibid.*). Here, Cassirer, then, allowed for narrower versions of Newton's principles to be applied to particular problem cases—without thereby undermining the principles as a whole—so long as the narrower version of the principle could be logically deduced from the universal version of the principle. So, for Cassirer, if anomalies within the classical framework, e.g. the advance of Mercury's perihelion, can be explained by special versions of Newton's laws—deducible from the universal versions—then the anomalies do not irrevocably break the "harmony between theory and experience".

So, if anomalies do not necessitate the adoption of new principles or laws, what does? Cassirer's answer is just that there will eventually be anomalies within the context of any given theory for which this path is not open: that is, where the suitable limitations of the applicability of the universal principles either cannot be deduced from the universals or cannot adequately account for the anomaly. The potential difficulty with such an approach should be apparent: how do scientists working within the context of a given theory know that they have exhausted these possibilities? Once the new theory, with new principles, is in place it may be clear that the account it provides of any phenomena once considered to be anomalous is superior to any account that could be offered in the previous theory: but what is there to motivate one to seek to develop the new theory in the first place rather than seeking to amend the current theory?

Cassirer argued that there are a number of possible considerations that would justify abandoning one such stopping-point for another:

(a) As we have seen, Cassirer argued that a study of the history of science demonstrated that scientific theories are seeking to elucidate the ultimate invariants of experience. This means that we must require of any new theory that it leaves certain key features of experience unaffected

(b) Cassirer argued that certain conceptual developments are brought about by seeking to unify our physical concepts (ETR, p.360). Cassirer stressed that this was a purely regulative demand because "true unity is never sought in things as such, but in intellectual constructions" (ETR, p.361).

(c) Cassirer also argued that certain principles of theory selection play a role in explaining the rationality of theory change. These principles are just the demand that mathematically simple (SF, p.260) and general (ETR, p.360) theories are to be preferred. The demand of generality was especially important because it was this that Cassirer took to mean that we should expect new theories to incorporate and explain the success of old theories—often by treating the old theory as a limiting case of the new.

Each of these is a regulative demand and, for Cassirer, this was sufficient to provide an account for the rationality of theory change. Thus, for example, he argued that the equivalence principle of general relativity came about as an attempt to unify the concepts of energy and matter (ETR, p.401) and he argued that part of the appeal of both special and general relativity is that they incorporated the earlier, classical, theory of space-time as a limiting case (ETR, pp.378-9).

This, I think, goes some way towards addressing Friedman's concerns, however there remains a significant problem for Cassirer's account of the rationality of science: what if objectivity-in-general is radically different from objectivity-at-a-time? Science could, in future, develop in a quite different direction from that which it has taken so far; what is currently taken to be objective in science may eventually come to be seen as not even a loose approximation of objectivity-in-general. Furthermore, if we have no guarantee that objectivity-at-a-time does approximate objectivity-in-general, how can it provide a secure foundation for the analysis of scientific knowledge?

Cassirer, I think, must accept that objectivity-at-a-time may fail to approximate objectivity-in-general but this is not necessarily fatal to his approach. Cassirer's central claim is that scientific knowledge is the most secure form of knowledge: he recommends only that we look at the history of scientific knowledge to date and try to identify features of scientific thought that remain constant throughout the development of scientific theories. If there were to be a radically new type of scientific theory developed, then Cassirer's approach would recommend reassessing the connection between the new scientific theory and our current theories to identify alternative candidates for the concept of objectivity-at-a-time that are capable of approximating the new understanding of objectivity-in-general at each successive stage of the sequence of scientific theories.[160] This difficulty for Cassirer, then, is just a consequence of his belief that scientific knowledge is the most secure form of knowledge available to us: even though it is liable to—potentially radically—change, the task of the philosopher is to understand how each stage of the development of scientific theories captures some relevant feature of state-of-the-art scientific knowledge.

The regulative framework for answering CR is, then, potentially very useful. However, it is not clear to me that it can be the full story. Friedman is essentially right in his objection that a purely regulative framework cannot explain how new theories become coherent possibilities. It seems that we would expect on Cassirer's account that a broader invariance group alone is a good enough motivation for theory change: however the prominent examples of theory change all involve changes on a less fundamental conceptual level first and then a broadening of the invariance group later.[161] This represents the chief advantage of Friedman's account over Cassirer's, he pays more attention to the role that the development of physical principles plays in the development of new scientific theories. In the next chapter I show how, in fact, aspects of both Friedman's and Cassirer's accounts are needed in order to make sense of the actual historical development of general relativity.

Cassirer's answer to CR, then, can be summarised as follows. The most important idea is that of the ultimate invariants of experience, which are the elements of cognition that we will ultimately—at the idealised end-point of science—judge to have been present throughout the history of science. These serve the function of ensuring that all the scientific theories in a sequence are all investigating the same subject: i.e., they are all engaged in the project of elucidating these fundamental invariants. These, I argued, were a priori only in a regulative sense because they could not be understood as constitutive either of individual theories or the sequence of theories. However, as Friedman points out, this alone does not suffice to give an explanation of the forward-looking rationality of theory change: it does not suffice, that is, to explain why, e.g., a Newtonian physicist would have been rational in accepting the theory of relativity. Cassirer suggested regulative a priori principles of theory selection and systematic unity to try and explain this aspect of theory change. The regulative a priori as a whole defined the concept <objectivity> for Cassirer. I have also identified a role for invariance principles as instantiating a concept of <objectivity-at-a-time> for any given scientific theory.

160 This would work in a similar fashion to Cassirer's own discussion of classical physics: within the tradition of classical physics it would not have obvious that it was capturing objectivity-at-a-time through its Galilean invariance. This could only become clear from the perspective of the theory of relativity. This, for Cassirer, is precisely why his position remains meaningfully Kantian even while his account refutes central aspects of Kantian philosophy.

161 This will be especially clear in the discussion of the development of general relativity in ch.4.

Cassirer, I think, successfully resolved the first of the two branches of his version of CR. The regulative a priori, in defining a concept of <objectivity>, provided a clear goal that science could be understood as aiming at. However, I do not think that the regulative a priori alone is enough to explain the rationality of abandoning a given theory in favour of another. The reason is that a broader invariance group, on its own, does not necessarily make a theory a preferable. This is because, as we will see in detail in the next section, invariance groups do not provide a theory with a physical interpretation. The physical interpretation is provided by constitutive principles and it is these that need to change—just as Friedman suggests—in order to drive the development of a new theory.

In the rest of the chapter I develop an account of constitutivity—grounded in Cassirer's group-theoretic understanding of mathematics—that is consistent with Cassirer's emphasis on regulative principles.

3.3.2.2. Structuralism and the constitution of objects

In this section I argue that there are two ways in which Cassirer's regulative approach is able to answer CC. First, on Cassirer's account laws are constitutive of the objects of a theory and, second, physical principles play a historical role in making laws possible. In order to understand Cassirer's account of constitutivity, we must first clarify the role of the a priori in his philosophy. I argue—with Richardson (1998), Ryckman (2005) and Heis (2012)—that Cassirer understood the a priori to function both constitutively and regulatively. Second, it is now commonplace in the literature to treat Cassirer as having anticipated the central ideas of structural realism, in the sense that Cassirer advocated a structural account of the constitution of objects.[162] Having identified the two different senses of the a priori in Cassirer's account—and how they stem from his particular understanding of the function-theory of concepts—it is possible to precisely clarify the sense in which Cassirer offers a structuralist account of constitution.

In the previous section I emphasised the regulative role of the a priori in Cassirer's philosophy. The regulative a priori featured in Cassirer's work in three ways: the ideal of the ultimate invariants of experience, certain principles of theory selection and a principle of systematic unity. However, Cassirer also identified aspects of science that are, I think, better understood as constitutive in a relative sense. Cassirer, I suggest, understood certain physical concepts and principles as playing a constitutive role in particular scientific theories. This can be seen quite clearly in his treatment of the theory of relativity.

[162] See (Ihmig, 1999), (Gower, 2000), (French, 2001) and (Cei and French, 2009).

As we have seen, for Cassirer the concept <objectivity> is the starting point for the analysis of science: our experience of objects is ultimately to be explained in terms of the objectivity of science. However, we have seen that for Cassirer, as for Kant, the objectivity of science is not simply a brute fact: it stands in need of explanation itself. Kant understood objectivity as resulting from the manner in which the manifold of sensibility was ordered under the categories of the understanding by means of spatial and temporal schema. This is why mathematics could so effectively describe objective reality, for Kant: geometry and arithmetic are related to the pure intuitions of space and time respectively. This is why, for Kant, natural science must be mathematical. The laws and objects of natural science are, as we saw in §2, for Kant then explained in terms of the objectivity of our experience of nature.

Cassirer, since he rejected the faculty of intuition, cannot understand objectivity in the same way. This is the role of the regulative a priori for Cassirer: it provided a way to interpret objectivity within a sequence of dynamically developing scientific theories. The regulative a priori provided Cassirer with a stock of concepts that were common throughout the evolution of knowledge: it is these that are ultimately objective. The problem with this, of course, is that we will only know those elements of thought that were truly objective when science reaches its ideal end-point. Until that time we must find a way to encode the concept <objectivity> in a given scientific theory. It is at this point that Cassirer, influenced by the group-theoretic approach in geometry, came to understand objectivity-at-a-time as being given by the invariant properties of a group of transformations. This is why general covariance was so epistemologically significant from Cassirer's perspective: it provided a broader invariance group than had previously been available and, as such, was understood as providing a better approximation of the ultimate invariants of experience.

However, there is a well-known problem with viewing general covariance as the fundamental statement of general relativity: it is not clear that there is any physical content to general covariance. This was an objection first raised by Kretschmann to Einstein's (1916) formulation of the theory of relativity.[163] Einstein understood general covariance just as the claim that the laws of physics have the same form in any coordinate system: in this sense he understood it as the culmination of his project to extend the principle of relativity to accelerating frames of reference.[164] Einstein was, though, well aware that this requirement was too weak to provide any physical content to the theory, and so he argued

163 See (Norton, 2003) for a detailed discussion of Kretschmann's objection.

164 I discuss the importance of the idea that the principle of relativity should be extended to incorporate non-inertial frames of reference in the next chapter.

that the physical content of a theory was given by the catalogue of coincidences that are invariant under coordinate transformations.

Kretschmann's objection was that the claim that the physical content of a theory is given by the catalogue of space-time coincidences is not a unique feature of general relativity: he claimed that any space-time theory can be given such a generally covariant formulation. This was a problem that Cassirer was aware of and sought to provide a solution to. Cassirer understood this type of objection to amount to the claim that general covariance is an analytic assertion in the sense that it is simply specifying what is meant by a universal law (i.e. one that is not changed in form by arbitrary changes of space-time variables) (ETR, p.384). However, general covariance insofar as it applies to physics should be understood as the demand that there *are* laws that are invariant through coordinate transformations, which is a synthetic claim (*ibid.*). General covariance, then, was understood as a regulative demand placed upon the structure of physical laws.[165]

Cassirer's understanding of objectivity, then, is squarely based in the regulative a priori. That which is ultimately objective is that which serves as a premise in every valid judgment throughout the development of science: i.e., the ultimate invariants of experience. The ultimate invariants of experience are a priori because they are *necessary* premises and regulative because uncovering the ultimate invariants of experience is that which is set as task for science. Invariance principles represent constraints that are placed on the form of the laws of a theory. They are regulative in the sense that they are intended to remove science from the parochial realm of individual observers and, in so doing, provide an approximation of the ultimate invariants of experience.[166]

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165 As such, Cassirer understood general covariance in the same way as he had understood Lorentz invariance, i.e. as "a general *maxim*…for the investigation of nature" which serves as a "heuristic aid in the *search* for the general laws of nature". Here, I think it is clear that the invariance principles serve as regulative demands: i.e. they do not make the laws of a theory possible—physical laws are possible without an underlying idea of an invariance group—instead they serve just to place formal limitations of the structure of the laws. It is for this reason, *contra* Ryckman, that I do not think that general covariance is constitutive of general relativity. I discuss the role of general covariance in general relativity in depth in §4.4.

166 I do not think that Cassirer understood the concept of <objectivity> as *constitutive* of the concept <object>: this is in contrast to the reading of Ryckman (2005, p.42ff.), who argues that Cassirer understood general covariance as an "objectifying unity" that played both a constitutive and regulative role. This is because, an invariance group, on its own, provides us only with a mathematical structure to which the laws of a theory must correspond: it does not tell us to what this mathematical structure corresponds. An invariance group just provides certain limitations on what can be posited of the objects of a theory: invariance groups tells us nothing positive of the properties of the object that satisfy the invariance nor of the types of law-like relationship that the objects enter into. The role of the constitutive principles, then, is to guide the conceptual construction of objects in accord with the constraints of a given invariance group. This is close to the understanding of permutation invariance defended by Massimi (2011) in the context of quantum theory. It is argued that permutation invariance alone permits both Pauli-obeying quarks and Pauli-violating paraparticles: contemporary physics supports a belief in quarks rather than paraparticles because experimental evidence supports predictions about the behaviour of quarks.

Let us examine how Cassirer provides an account of constitutivity by means of an example: the equivalence principle. In ETR, Cassirer described the role of the equivalence principle in the following fashion:

> We conceive ourselves in the position of an observer, who, experimenting in a closed box, establishes the fact that all bodies left to themselves move, always with constant acceleration, toward the floor of the box. This fact can be represented conceptually by the observer in a double manner: in the first place, by the assumption that he is in a temporarily constant field of gravity in which the box is hung up motionless, or, in the second place, by the assumption that the box moves upward with a constant acceleration whereby the fall of bodies in it would represent a movement of inertia. The two: the inertial movement and the effect of gravitation, are thus in truth a single phenomenon seen and judged from different sides. It follows that the fundamental law that we establish for the movement of bodies must be such that it includes equally the phenomena of inertia and those of gravitation. As is seen, we have here no empirical proposition abstracted from particular observations, but a rule for our construction of physical concepts: a demand that we make, not directly of experience, but rather of our manner of intellectually representing it. (ETR, p.428)

Cassirer introduced the equivalence principle here in terms of Einstein's elevator thought experiment: if we experiment in a closed environment, with no access to anything beyond our immediate surroundings, we can explain the fact that objects accelerate towards earth either by postulating an appropriate gravitational field or by assuming that we are actually accelerating upwards ourselves. This, Cassirer argued meant that both inertial and gravitational effects were two different ways of "intellectually representing" the same phenomenon. This is not an empirical claim: the equivalence principle is, instead, a rule that is imposed upon the construction of general relativity in order to ensure that it captures the idea that inertial effects can be represented as gravitational effects.

This served, for Cassirer, to provide an interpretation of general covariance. General covariance meant that the physical law—the field equations—must have the same form when viewed from any coordinate system. In order to ensure that this invariance principle has physical meaning, some additional claim is required. The equivalence principle was able to play this role, for Cassirer, because it provided Einstein with a way to treat two physically distinct systems as being described by the same law. That is, it provided a way to understand the physical concepts of inertia and gravitation such that it made physical sense to use the same form of the physical law for two seemingly distinct physical situations. So, the equivalence principle was constitutive of Einstein's field equations in the sense that it made it possible to physically interpret that demand of general covariance.

In §4 I examine the role of the equivalence principle in the development of general relativity in more depth. I argue that Friedman is essentially correct in taking the equivalence principle to play this role in virtue of its historical role in the development of Einstein's field equations. This approach, I think, is a little different from Cassirer's as detailed in ETR, where Cassirer seems to take the equivalence principle to directly construct a conceptual framework within which to make sense of the demand for general covariance. However, it is consistent with his account of the role of physical principles in DIMP.

Here Cassirer argued that science could not be concerned solely with the process of gathering together empirical observations or data and generalising these observations into laws.[167] First, in doing so it would be hard to see how we could overcome the problem of induction. Second, the process simply does not reflect the practice of science: scientists are not satisfied simply with generating a list of generalisations, they also seek to explain how laws relate to each other. Instead, science progresses by seeking "a rule by which it can be guided from one law to the next" (DIMP, p.45): this is the role of physical principles such as the equivalence principle. In §4 I argue that the equivalence principle is a particularly good candidate to be understood as constitutive of general relativity in this sense because it is initially taken from Newtonian physics and serves to guide the development of general relativity out of Newtonian physics.

This, I think, provides a means to understand the constitutive role of the equivalence principle in a manner that is close to Friedman's, but without some of the associated baggage. For Friedman, the purpose of constitutive principles is to make mathematical possibilities physical possibilities, e.g., to make physical sense of the idea of four-dimensional space-time. Friedman's Kantianism is premised upon the idea that there is something mysterious about the application of mathematics to experience. Cassirer understood the relation between mathematics and physics quite differently: he, following Cohen, sought to preserve Kant's idea that mathematical statements are true in virtue of their application in experience to describe the behaviour of empirical bodies.[168] So, rather than viewing mathematics as purely a system of implicit definitions devoid of physical

[167] See §3.3.2.1

[168] "Although we know *a priori* in synthetic judgments a great deal regarding space in general and the figures which productive imagination describes in it, and can obtain such judgments without actually requiring any experience, yet even this knowledge would be nothing but a playing with a mere figment of the brain, were it not that space has to be regarded as a condition of the appearances which constitute the material for outer experience. Those pure synthetic judgments therefore relate, though only mediately, to possible experience, or rather to the possibility of experience; and upon that alone is founded the objective validity of their synthesis" (Kant, A157/B196). Kant's idea, then, was that while mathematical judgments are obtained through construction in pure intuition, they count as cognitions only because they are necessarily connected to experience in the sense that geometrical space wars understood as a condition of appearances.

content that needs to be connected to experience, Cassirer understood mathematics as "essentially applicable" to physics.[169] This means that the regulative constraint on the mathematical form of a theory's laws and the manner in which constitutive principles serve to add physical content to the laws work together to account for knowledge of empirical laws. Constitutive principles do not make "mathematical possibilities physical"; they simply add physical content to a regulative constraint on the form of the laws.

This, then, is how Cassirer grounded the empirical laws of a theory: their form is determined by the regulative demand of objectivity and their physical content is constructed by constitutive principles. Cassirer, in addition, argued that the objects of a theory are constituted by the laws of a theory. As we see in §5, this has become the subject of significant contemporary interest because, in advocating the law-constitutive account of objects Cassirer advocated a position that has much in common with modern structural realism. In the rest of this section I seek to clarify the sense in which Cassirer's provided a structural account of the constitution of objects.

That Cassirer advocated a law-constitutive account of the concept <object> is most clear in DIMP, though it is also apparent in ETR. Since the contemporary discussion focuses on Cassirer's law-constitutive account of objects in quantum mechanics, I will use Cassirer's argument in DIMP to illustrate his position. In DIMP, Cassirer explicitly states that he understands laws to be conceptually prior to objects:

> The concept of law is now regarded as prior to that of object, whereas it used to be subordinate to it. In the substantialistic conception there used to be a definitely determined entity which bore certain attributes and which entered, with other entities, into definite relations expressible by laws of nature. In the functional viewpoint, by contrast this entity constitutes no longer the self-evident starting point but the final goal and the end of the considerations: the *terminus a quo* has become a *terminus ad quem*" (p.131)

For Cassirer, then, there are no objects in the sense of substances that bear physical properties: instead objects emerge only through an interweaving of physical principles, laws of nature and empirical measurements. In classical physics, objective knowledge had been

[169] The argument that Cassirer understood mathematics to be essentially applicable to physics is made in detail in (Heis, 2011a, §5). Cassirer made it most clear that this was how he understood the relationship between mathematics and physics in *Kant und die Moderne Mathematik*, where he claimed, e.g.: "If it is not possible to prove that the system of pure concepts of the understanding is the necessary condition under which we can speak of a rule and connection of appearances, and under which we can speak of empirical 'Nature' – then this system, with all its consequences and conclusions, would have to still appear as a mere 'figment of the brain' . . . *The logical and mathematical concepts should no longer constitute tools with which we build up a metaphysical 'thought-world': they have their function and their proper application solely within empirical science itself.*" (Cassirer, 1907, pp.42-3, translated in Heis, 2011a, p.785, my emphasis).

associated with knowledge of objects; Cassirer, by contrast, associated objective knowledge with permanence.[170] It is for this reason that Cassirer continues, in DIMP, to claim that "objectivity…is attained only because and insofar as there is conformity to law" (p.132). While substantial objects are not permanent in the way that Cassirer requires—their properties may change over time—the laws describing the changes in objects' properties *are* permanent within the framework of a particular theory. In turn, the relations expressed by the laws of a theory must be objectively grounded in the sense that they must be invariant under transformations of the permutation group of a particular theory. Objectivity is not connected to the existence of "things"; it is concerned with the "objective validity of relations" (p.143).

What, then, is a quantum object—e.g., an electron—for Cassirer? Cassirer is quite clear that an electron cannot be understood as an individual object and must, instead, be interpreted relationally:

> If then we continue to talk about the individuality of particles, this can only be done indirectly; not insofar as they themselves, as individuals, are given, but so far as they are describable a "points of intersection" of certain relations. (p.180)

It is clear that in quantum physics the corpuscular character of electrons must be abandoned: electrons extend throughout their configuration space and the charge of an electron is no longer confined to a particular location, instead it is spread across a "charge cloud". While the charge of an electron is indivisible, it does not seem to be the type of property that can be localized. Cassirer is critical of Sommerfeld's resistance to this idea (p.182): Sommerfeld, according to Cassirer's account, took the indivisibility of electronic charge to imply that electrons are point like entities. This view is mistaken because "the constancy of a certain relation is not at all sufficient for the inference of a constant carrier" (*ibid.*).

Cassirer clarified his conception of the electron by linking it explicitly to Kant's relational conception of objects according to which "All that we know in matter is merely relations, but among these relations are some self-subsistent and permanent, and through these we are given a determinate object" (Kant, A285/B341). We are in a position to consider the electron as a "determinate object" only because the indivisibility of electronic charge is such a self-subsistent and permanent relation. So, for Cassirer, we are in a position to consider electrons as objects only because they possess a certain property—

[170] This is most clear in SF, e.g. "We finally call objective those elements of experience, which persist through all change in the here and now, and on which rests the unchangeable character of experience" (p.273). This, of course, is why the invariants of experience are ultimately that which is objective: i.e., they are those elements of cognition that are permanent throughout the history of scientific knowledge.

charge—which enters into permanent relations with other entities and these relations are described by the laws of nature. That is, to identify determinate objects we must begin with the laws that express the relations from which objects are constituted.

This, then, is the second strand of Cassirer's account of constitutivity: the laws of physics have a relational form imposed upon them by invariance principles and, in turn, the objects of a theory are just the points of intersection of these relations. This is a structural account of the constitution of objects, then, in the sense that our knowledge of objects is only possible once they are embedded in a relational structure that is given by the laws of a theory.[171]

Cassirer's answer to CC, then, has two parts. First, there is a role for constitutive principles in physically interpreting a theory's invariance principles: in this sense they make possible a specific theory's empirical laws because without such an interpretation, invariance principles do not uniquely determine the laws of a theory. Secondly, in the fashion just detailed, the objects of a theory are (structurally) constituted by theory's empirical laws. In this way Cassirer advocated both an absolutely and regulative a priori and a relativised and constitutive a priori. Both have a role to play in understanding his account of constitution: the regulative a priori determined the form of the laws while the constitutive a priori determined the content of the laws.

3.3.3. Cassirer, structure and reason: a summary

Cassirer's philosophy of science, then, can be summarised as follows:

i. Cassirer interpreted mathematics group-theoretically and, as such, understood it as straightforwardly applicable to experience.
ii. The fundamental distinction between the substance-theory of concepts and the function-theory of concepts should be understood in terms of a reversal of the explanatory priority of the epistemological concepts <object>, <truth>, <knowledge>, <objectivity>. On the substance-theory of concepts <objectivity> is explained in terms of <object>, while on the function-theory of concepts <objectivity> is taken as that which is fundamental and the possibility of the objects of experience is explained in terms of objectivity.

[171] I examine the relationship between Cassirer's structuralism and structural realism in §4, but for the time being it suffices to say that Cassirer himself does not advocate structural realism because the relational structure of the laws of physics is a consequence of his regulative understanding of invariance principles. Furthermore, the physical content of the laws is given by constitutive principles, which are just rules for the construction of physical concepts. This understanding of law makes it impossible for Cassirer to think that the objects of physics could refer to externally existing objects: this system is designed solely to ensure knowledge of the objects of experience.

iii. The concept of <objectivity>, understood as a regulative goal, ensures the unity of science: i.e. each scientific theory is interpreted as seeking to identify the necessary premises of every true statement throughout the development of science. This—along with other regulative principles—is supposed to ensure the rationality of the development of science.

iv. At any given stage of science <objectivity> is approximated by invariance principles. An invariance principle should be understood as a regulative demand that is imposed upon the mathematical form of a law. This, though, is not sufficient to provide laws with physical content and for that, constitutive principles are required.

v. Finally, objects are reconceptualised as the points of coincidence of the various relations described by a theory's laws. Thus, Cassirer offered a structuralist, law-constitutive account of the knowledge of objects.

3.4. A regulative account of constitution

Where, then, does the discussion of this chapter leave us? The discussion of Cassirer's SF, ETR and DIMP has left us with alternative Kantian approaches to answering CR and CC. The characteristic feature of Cassirer's analysis of science is that the concept <objectivity> should be understood as the fundamental epistemological concept. This means that the truth of any statement in the context of a scientific theory has ultimately to be explained in terms of the concept objectivity. There were two senses in which Cassirer understood objectivity. First, there was the idea of objectivity in general: this refers to the ultimate invariants of experience that are the necessary premises of every scientific claim. Objectivity in this sense represents the idealised end-point of scientific inquiry. I identified another sense of objectivity that was important to Cassirer's philosophy: he argued that invariance principles provide a concept of <objectivity-at-time> with respect to which the statements of individual scientific theory are ultimately to be explained.

Cassirer answered CR as follows. Science is directed at uncovering the ultimate invariants of experience. This, in light of Cassirer's emphasis on historicising Kant's transcendental logic, is important because it ensures that all scientific theories are concerned with the same object of study. The ultimate invariants of experience, though, are just one of the absolute and regulative aspects of the a priori that Cassirer identifies: he also suggests that certain principles of theory change and a principle of systematic unity are regulative a priori principles. The regulative a priori is supposed to provide a means of explaining the forward looking rationality of science. In particular, Cassirer would argue that it is rational to abandon one theory for another if the new theory has a broader invariance group, is more unified or, e.g., more mathematically simple. This insight, I think, can serve as the foundation for a Kantian account of the rationality of science, but it is not yet entirely satisfactory. This is an important part of the regulative answer to CR. I suggest that in order to secure the *prospective* rationality of the development of a new scientific theory, we need to also appeal to the historical role that physical principles play in driving the development of new theories: in light of the discussion of Cassirer's understanding of the constitutive role of physical principles in §3.3.2.2, I take it that Cassirer's account can make sense of this feature of the development of new theories.

Cassirer's account of constitutivity has two parts. First, he required that there were constitutive principles that served to give physical content to the laws of a theory, whose form was restricted by invariance principles. Second, he defended a law-constitutive account of objects. In this, I think Cassirer was essentially correct and in §5I will show that general relativity can be profitably understood in this fashion.

In the next chapter I turn my attention towards the development of general relativity and seek to show that the historical process by which the theory emerged can be profitably understood in regulative terms. In particular, I am concerned to show that the regulative approach provides a better account of the rationality of the development of general relativity than Friedman's account, on which philosophy plays a crucial, meta-paradigmatic role.

4

Regulative principles and the rationality of theory change

The equivalence principle in the development of general relativity

4.1. Introduction: the equivalence principle and general relativity

Cassirer's account of the rationality of scientific theory change provides us with a new Kantian framework within which to answer CR. In this chapter I seek to show that the regulative answer to CR makes better sense of the development of general relativity than Friedman's constitutive approach does. In particular, I take issue with two of Friedman's main claims: (1) that the adoption of general relativity over Newton's theory of gravitation was rational on account of Einstein's fruitful interaction with the philosophy of Helmholtz and Poincaré and (2) that the equivalence principle is constitutive of general relativity in the sense that it—with the rotating disk thought experiment—made a variably curved four-dimensional space-time a genuine physical possibility.

My analysis focuses on two key features of the development of general relativity. I begin by examining the role of the equivalence principle in the early stage of the development of general relativity between 1907 and March 1912. There are two features of this analysis that are of significance. First, I seek to counter Friedman's claim that Einstein placed so much emphasis on the equivalence principle because he self-consciously elevated an empirical fact to the status of a principle in the manner of Poincaré. Second, I argue that the emergence of—and Einstein's persistent emphasis on—the equivalence principle can more profitably be understood in regulative terms. That is, I argue that it is a principle taken from Newtonian physics that played a crucial historical role, via its broadening of the admissible class of coordinate transformations, in the development of the new laws of general relativity.

I then turn my attention towards the role of the rotating disk thought experiment in the development of general relativity. As in the preceding discussion of the equivalence principle, my analysis of the rotating disk has both a negative and a positive aim. First, I seek to argue, *contra* Friedman, that there is no need to appeal to Helmholtz's philosophy in order to understand the role of the thought experiment in the development of relativity. A central part of my argument to this end is to provide an alternative reading of 'Geometry and Experience' to that offered by Friedman, as on Friedman's interpretation this lecture—as we saw in §1.2.2—provides a strong motivation for thinking that Einstein did utilise Helmholtz's empiricist account of geometry in developing general relativity. I argue that 'Geometry and Experience' should be understood primarily as Einstein engaging in a contemporary debate between Reichenbach and Weyl about the status of the line element in general relativity: as such, I argue that this lecture does not provide a firm basis for Friedman's claim that Helmholtz's epistemology of geometry played a crucial role in the development of general relativity.

I also develop an alternative account of the role of the rotating disk thought experiment in general relativity, arguing that it—like the equivalence principle—served a regulative function in that it broadened the group of coordinate transformations that Einstein could consider. In doing so I seek to clarify that if, in accord with Cassirer's regulative Kantianism, we adopt group-theoretic methodology then there is no mystery about how four-dimensional space-time became a physical possibility. This, I suggest, undermines Friedman's particular understanding of the constitutive a priori and means that—while we can salvage much of Friedman's insight into the role of the equivalence principle in the development of general relativity—the constitutive role of the principle is better understood in regulative terms.

Finally, I turn my attention towards examining the role of general covariance in general relativity. Here, my main concern is to clarify the sense in which Cassirer's took general covariance to be "*the* epistemically salient aspect of the general theory of relativity".[172] In particular I seek to clarify the sense in which general covariance serves as a regulative principle that defines a notion of <objectivity-at-a-time> within general relativity. I also argue, against Ryckman, that general covariance should not be understood as a constitutive principle.

This chapter, then, is intended to be my main argument that a Kantian philosophy of science that emphasises the role of regulative principles is in a stronger position to answer CR than one that emphasises constitutive principles. In the following chapter I then turn to one remaining concern: that the account of constitutivity associated with this account of general relativity might be more naturally understood in structural realist rather than Kantian terms.

[172] (Ryckman, 2005, p.46)

4.2. The development of the equivalence principle: elevation or regulative demand?

The development of general relativity can be divided into two phases: in the first phase (1907-1912) Einstein sought to develop a relativistic theory of gravitation that was adequate for the description of static gravitational fields, in the second phase (1912-1915) Einstein sought to extend his account of static fields so that it would apply to the more complex case of stationary fields. In this section I examine the role of the equivalence principle in the first of these two stages.

The first thing to stress is that there is not really any such thing as *the* equivalence principle: a number of physical principles get referred to as the equivalence principle and, as we will see, Einstein himself uses at least three different versions of the principle. The version of the equivalence principle that Friedman argues is constitutive of general relativity is the following:

> [Gravitational] force is actually of the same kind as the so-called inertial forces (such as centrifugal and Coriolis forces) arising in non-inertial frames of reference, in so far as the accelerations produced by both types of forces are entirely independent of the bodies so affected. (Friedman, 2010a, p.659)

In part, Friedman's answer to CR, depends on the claim that Einstein understood this version of the equivalence principle as the result of his "elevating" an empirical fact to the status of a principle. The first goal of this section, then, is to show that this analysis does not do justice to certain facts about Einstein's work in the period 1907-1912. In particular, I show that (i) this version of the equivalence principle is the third that Einstein used and that it emerged via a process of conceptual analysis from the previous two versions of the equivalence principle and (ii) there is no clear historical motivation to suppose that Einstein understood even the first version of the equivalence principle that he used as a convention. This, I suggest, means that it is unnecessary to appeal to Einstein's adopting Poincaré's methodology in order to explain the rationality of the development of general relativity. Furthermore, I argue in §4.2.3 that the development of the equivalence principle receives a natural alternative interpretation within the framework of a regulative Kantianism.

4.2.1. *1907: The Newtonian statement of the equivalence principle*

In 1907 Einstein began work on developing a relativistic account of gravitation: by extending the relativity principle so that it applied to uniformly accelerating frames of reference, he was able to predict that the speed of light would vary in a gravitational field and would seem to bend around massive bodies.[173] This extension of the relativity principle—though he did not yet refer to it as such—was Einstein's first statement of the equivalence principle. In this section I am concerned with two questions: (1) how did Einstein come to be convinced that the key to developing a relativistic account of gravitation lay in extending the relativity principle? (2) What was the role of the extended relativity principle in the argument of Einstein's (1907)? In response to the first question, I argue that Einstein took the equivalence principle form Newtonian physics in order to ensure that his new theory did not violate the universality of free-fall. Second, I argue that Einstein understood the variability of the speed of light as an inconsistency between Newtonian physics and special relativity. As we see in the next section, Einstein's 1912 theory of static gravitational fields, should then be understood as an attempt to resolve this apparent inconsistency.

The equivalence principle, as it features in Einstein's (1907) can be stated as follows:

(EP1): The laws of nature take the same form in a uniformly accelerating frame of reference as they do in a stationary frame of reference equipped with a homogeneous gravitational field.

Let us begin our discussion of the equivalence principle EP1 by examining its early role in Einstein's thought. EP1 was initially introduced as an extension of the relativity principle:[174]

> Hitherto we have applied the principle of relativity, i.e., the assumption that the laws of nature are independent of the state of motion of the reference system, only to systems of reference free of acceleration. Is it conceivable that the principle of relativity is also valid for systems that are accelerated relative to each other? (1907, CPAE 2, p.302)[175]

Using Σ to denote a resting frame with a homogeneous gravitational field and Σ' to denote a uniformly accelerating frame, Einstein then stated EP1 as follows:

[173] This early prediction would, of course, be confirmed by Eddington in 1919.

[174] The relativity principle is just the claim that physical laws take the same form with respect to all inertial frames.

[175] For referencing Einstein's works I follow a different convention from that which I have adopted previously. I refer to their year of original publication and additionally give the volume and page number referred to in *The Collected Papers of Albert Einstein* where appropriate, rather than referring to the year of publication of the version of the translated version of the papers.

> As far as we know, the physical laws with respect to Σ do not differ from those with respect to Σ'; this is based on the fact that all bodies are equally accelerated in the gravitational field. At our present state of experience we have thus no reason to assume that the systems Σ and Σ' differ from each other in any respect…This assumption extends the principle of relativity to the uniformly accelerated translational motion of the reference system. (*ibid.*)

The most basic intuition behind EP1, then, is just that a physical situation described with respect to a uniformly accelerating frame of reference is described in exactly the same way with respect to a stationary frame of reference equipped with a homogeneous gravitational field. The sense in which this extends the relativity principle is quite clear: the equivalence principle extends the relativity principle so as to include a particular class of non-inertial frames (i.e. those that are uniformly accelerating).

Why, though, did Einstein seek to introduce gravitation by extending the relativity principle? Einstein introduced EP1 in his (1907) without really providing any concrete explanation as to what motivated the principle: all that he says is that EP1 provides the most natural way to extend the relativity postulate. Furthermore, a study of Einstein's correspondence from around this period reveals little that gives us any extra insight. So, in order to understand the thought process behind the introduction of the equivalence principle we must rely on the account that Einstein offers in his memoirs.[176]

Einstein's most detailed memoir of the process which led him to the introduction of the equivalence principle—which is also emphasised by Norton (1995, 2007b)—is his (1954 [1933]).[177] Here, speaking of the problem of extending the principle of relativity, Einstein wrote:

> I came a step closer to the solution of the problem when I attempted to deal with the law of gravity within the framework of the special theory of relativity. Like most writers at the time, I tried to frame a *field-law* for gravitation, since it was no longer possible, at least in any natural way, to introduce direct action at a distance owing to the abolition of the notion of absolute simultaneity
>
> The simplest thing was, of course, to retain the Laplacian scalar potential of gravity, and to complete the equation of Poisson in an obvious way by a term differential with respect to time in such a way that the special theory of relativity was satisfied. The law of motion of the mass point in a gravitational field had also to be adapted to the special

[176] It is, of course, well-known that the accounts of scientists looking back on their own work can be unreliable: however, in this case, the relevant memoirs are quite detailed and coherently fit with Einstein's writing and correspondence from the period in question.
[177] The importance of this memoir is also stressed by Norton (1995; 2007b).

> theory of relativity. The path was not so unmistakably marked out here, since the inert mass of a body might depend on the gravitational potential. In fact this was to be expected on account of the principle of the inertia of energy. (1954, pp.286-7)

So, Einstein initially attempted to get a relativistic field-equation for gravitation by amending the Poisson field-equation for gravitation so as to render it consistent with the results of special relativity. The most natural way to do this is simply to exchange the Laplacian operator that appears in the classical Poisson equation with the Lorentz covariant D'Alembertian operator.[178] This much, it would seem, was relatively unproblematic. However, it would seem, that there were more serious difficulties here in terms of understanding the equations of motion for a body in a gravitational field, with Einstein's somewhat cryptic remark that "the inert mass of a body might depend on the gravitational potential".

What might this mean? Norton (2007b, pp.418-9) offers a plausible interpretation. The problem occurs when we try to derive relativistic, field-theoretic equations that govern the behaviour of masses in a relativistic gravitational field. In particular such an approach turns out to be possible only in cases where the scalar potential of the gravitational field is constant along the trajectory of the particle: as such, this approach is suited to describe only a very limited group of gravitational fields. Nordström suggested that this difficulty could be overcome if the mass becomes a function of the potential. This is, in all probability, what Einstein had in mind when he claimed that the inertial mass may depend on the gravitational potential.[179]

Einstein's attempt to understand the motion of a mass particle in a relativistic gravitational field, then, seemed to raise some quite serious problems. Einstein's memoir continues:

> These investigations, however, led to a result which raised my strong suspicions. According to classical mechanics, the vertical acceleration of a body in the vertical gravitational field is independent of the horizontal component of its velocity. Hence in such a gravitational field the vertical acceleration of a mechanical system or of its centre of gravity works out independently of its kinetic energy. But in the theory I advanced, the acceleration of a falling body was not independent of its horizontal velocity of the internal energy of the system. (1954, p.287)

Einstein's concern here is clearer. Renn (2007, p.55) suggests a thought experiment that neatly encapsulates this problem: consider a stone falling vertically and a projectile that is

[178] Norton (2007b, p.417) also suggests that this may have been what Einstein had in mind here.
[179] See Norton (2007b, p.419)

fired horizontally from the same height at the same time that the stone is dropped, then, in classical mechanics we expect the two bodies to hit the floor simultaneously. We can consider this situation from a reference frame that is moving at the same speed as the projectile but in the opposite direction, so that the roles of the stone and the projectile appear to be reversed. Both bodies are still expected to fall to the floor at the same time. However in special relativity the matter is not so simple. The difficulty is that what is simultaneous is meant to be frame dependent; this means that if two events are simultaneous in one frame then they should not be simultaneous in another frame. This means that the equivalence of gravitational and inertial mass would have to be violated.

Einstein then went on to explain that this violation of the principle that all bodies fall with the same acceleration within a gravitational field led him to abandon the approach of developing a Lorentz covariant gravitational theory:

> This did not fit with the old experimental fact that all bodies have the same acceleration in a gravitational field. This law, which may also be formulated as the law of the equality of inertial and gravitational mass, *was now brought home to me in all its significance*. I was in the highest degree amazed at its existence and guessed that in it must lie the key to a deeper understanding of inertia and gravitation. I had no serious doubts about its strict validity even without knowing the results of the admirable experiments of Eötvös, which—if my memory is right—came only later. I now abandoned as inadequate the attempt to treat the problem of gravitation, in the manner outlined above, within the framework of the special theory of relativity. It clearly failed to do justice to this most fundamental property of gravitation. (Einstein, 1954, p.287, emphasis added)

What this tells us is that prior to 1907, Einstein had sought to develop a relativistic account of gravitation according to which the gravitational field was an external field imposed upon space-time in the same sense as Newton's gravitational field was imposed upon Newtonian space. However, this approach towards developing a relativistic account of gravitation could not succeed because there was an incompatibility between a relativistic field-theoretic approach to gravitation and the experience of the universality of free fall.

After developing the special theory of relativity, then, Einstein sought to unify his theory of relativity and Newton's theory of gravitation. This most natural way to do so was by amending the three-dimensional Poisson equation for the gravitational field with a suitable relativistic candidate. However, he quickly saw that this method of proceeding would be hopeless: a relativistic field-equation for gravitation did not seem able to secure the universality of free-fall.

I suggest that Einstein traced this problem back to a problem with the concept of acceleration in Newtonian physics.[180] DiSalle characterises the process by which Einstein introduced the equivalence principle as one of *dialectical engagement* with the Newtonian concept of absolute acceleration. The process of dialectical engagement is related to conceptual analysis, but is intended to be stronger:

> To call this process a conceptual analysis, indeed, is to understate the force of the argument: it is typically a dialectical argument from the prevailing definition to a new one, revealing the hidden presuppositions of the old conception, and exhibiting the internal difficulties that must be resolved by the new. (DiSalle, 2010, p.528)

DiSalle, then, understands conceptual analysis as just the process of revealing the consistency or inconsistency of ideas. Dialectical engagement is intended to go deeper by revealing what it is in the conceptual content of the old theory that needs to be revised in the new theory.

What does it mean to say that the equivalence principle arises from Einstein's dialectical engagement with the Newtonian concept of absolute *acceleration* (2006, pp.120-31)?[181] If we consider a privileged centre of mass frame in Newtonian physics—DiSalle focuses on the system of Jupiter and its moons—in which all accelerations due to gravity are equal and in the same direction then, by Newton's Corollary VI (and Galileo's equivalence principle) we can treat this system as inertial. For DiSalle, this is a contradictory situation that arises within Newtonian physics: "different local inertial frames separately satisfy empirical criteria for being inertial frames, yet are non-inertial relative to one another" (p.130). We may locally treat, e.g. Jupiter and its moons as inertial and Saturn and its moons as inertial, yet—because the acceleration due to gravity on each will be different

[180] DiSalle (2006, §4.4) emphasises the importance of Einstein's dialectical engagement with the Newtonian concept of acceleration in developing general relativity. My account differs from DiSalle's in that he takes the process of Einstein's dialectical engagement with the concept of acceleration to be sufficient to get to develop the concept of curved space-time on its own. I think that the process was important for Einstein's identification of EP1 as the starting point for the development of a theory of relativity. However, as we will see, after that I suggest that EP1 was used to derive a different conceptual difficulty that needed to be resolved before the concept of four-dimensionally curved space-time could be arrived at.

[181] DiSalle also offers an insightful treatment of the development of the special theory. Einstein's crucial insight—the conventional definition of simultaneity—came about, on DiSalle's account, through Einstein's dialectical engagement with the Newtonian concept of simultaneity. Prior to Einstein, DiSalle points out, the concept of absolute simultaneity gains its empirical content through its relation to the Newtonian account of gravitational interaction. However, towards the end of the nineteenth century some physicists—DiSalle focuses on James Thomson—began to notice that this was deeply problematic as there was no way to empirically determine events to be simultaneous over great distances. Thomson was the first to point out that determination of simultaneity requires the use of signalling. On DiSalle's reconstruction, Einstein combined this thought with the fact that the two-way speed of light was known to be both finite and invariant and conventionally sets the one-way speed of light to be invariant and used signalling by this means to determine the simultaneity of any two events.

as Jupiter is nearer to the Sun—the two systems may be non-inertial with respect to each other. Einstein alone noticed that the concept of an inertial frame was not well-defined in classical physics in this way. So, for DiSalle, it was through the process of dialectical engagement that Einstein came to realise that the concept of an inertial frame was not well-defined in classical physics in this way.

If this is right and Einstein had indeed seen in 1907 that Corollary VI means that the concept of an inertial frame is not well-defined in Newtonian physics, then this sheds light on why he turned to EP1 to solve the problem that he described in his (1954 [1933]) memoir. To clarify this, though, we must first discuss the relationship between Corollary VI and EP1: in particular, I suggest that EP1 is equivalent to a combination of Corollary VI and Galileo's equivalence principle. Corollary VI states:

> *If bodies, any how moved among themselves, are urged in the direction of parallel lines by equal accelerative forces, they will all continue to move among themselves, after the same manner as if they had been urged by no such forces.*
>
> For these forces acting equally (with respect to the quantities of the bodies to be moved), and in the direction of parallel lines, will (by Law II) move all the bodies equally (as to velocity), and therefore will never produce any change in the positions or motions of the bodies among themselves. (Newton, 1729, p.25)

Here Newton claims that if bodies are all urged in the direction of parallel lines by equal accelerative forces then they can be treated as if they were not urged by any force at all. The primary application of Corollary VI in the *Principia* is to the system of Jupiter and its moons. Newton sought to determine the force exerted by Jupiter on its moons so that he would be able to determine the mass of Jupiter.[182] The difficulty that Newton faced in determining the central forces within the system of Jupiter and its moons is that the system is not isolated: the sun exerts an accelerative force on Jupiter and each of its moons. Before Corollary VI could be applied to this situation, Newton needed to appeal to Galileo's equivalence principle, which states that all bodies fall at the same rate in a gravitational field regardless of their mass. This is required to ensure that the Sun exerts an equal gravitational force on each of the bodies in the system. Then Corollary VI can be applied in order to permit Newton to treat Jupiter's system as inertial.

[182] This was important because it was the first stage in Newton's calculation of the centre of mass of the solar system. This was the central task of the *Principia*: i.e., to show that neither the earth nor the sun is the centre of the solar system and that, rather, the centre should be understood as the centre of mass, which is very close to the sun. See (DiSalle, 2006, §2.8) for detailed discussion.

From the perspective of the development of general relativity, this is important because the conjunction of Corollary VI and Galileo's equivalence principle is equivalent to EP1. In his treatment of the system of Jupiter and its moons, Newton began by considering the system as an accelerating one. Because the accelerative forces on the planet and its moons were gravitational, Newton could be sure that the outside accelerative forces were all equal and could therefore, by Corollary VI, be discounted. In effect, then, Corollary VI and Galileo's principle together enable Newton to claim that the motions of bodies among themselves are the same in an accelerating system as they are in a uniform gravitational field. The difference between this claim and EP1 is just that Newton refers to "bodies, anyhow moved among themselves" where EP1 refers to the form of the laws of nature and this, I take it, is not a substantial difference.[183]

This helps to clarify why Einstein took the equivalence principle to be of such central importance to the development of a relativistic theory of gravitation. Einstein (1954) claimed that he initially sought to develop general relativity as a relativistic field theory: however, he quickly saw that this contradicted "the old experimental fact that all bodies have the same weight in a gravitational field". In 1907, I suggest that Einstein saw this problem as a consequence of the fact that Newton's theory of gravitation and special relativity were inconsistent because special relativity relied upon the concept of an inertial frame while the concept was not well-defined in Newton's theory. EP1 would have seemed a promising candidate to ensure the consistency of the two theories. Corollary VI was the source of Einstein's realisation that the concept of an inertial frame is not well-defined in classical physics. By placing EP1—which incorporates Corollary VI—at the centre of his developing theory, Einstein could ensure that his new theory would not privilege inertial frames of reference in the same way that the special theory of relativity did. Furthermore, since EP1 *also* incorporates Galileo's equivalence principle, Einstein could ensure that in his new theory all bodies would have the same weight in a gravitational field. While Newton's theory contained an insight that was equivalent to EP1, it was not able to provide an interpretation of the insight: i.e., it remained a mystery as to how inertial and gravitational

[183] The claim that a version of the equivalence principle is equivalent to the conjunction of Galileo's equivalence principle and Corollary VI is common in the literature: for noteworthy recent examples see (Disalle, 2006) and (Saunders, 2013). Saunders (p.37) is particularly helpful in understanding the relationship between Corollary VI and the equivalence principle. He shows that the conjunction of Corollary VI and Galileo's equivalence principle can be states as follows: "If bodies moved in any manner among themselves are urged in the direction of parallel lines by equal gravitational forces due to outside bodies, they will all continue to move among themselves, after the same manner as if they had not all been urged by that force" (*ibid.*). Saunders then offers the following statement of the equivalence principle—which is equivalent to my EP1—that makes the relationship between this principle and Corollary VI clear: "If bodies moved in any manner among themselves are described in relation to an accelerating but nonrotating frame of reference, they will all move in relation to that frame as if acted on by uniform gravitational forces, producing the opposite acceleration" (*ibid.*).

mass could be equal while also being conceptually distinct. By beginning his investigation into general relativity with EP1, Einstein could solve the problem of the inconsistency of special relativity and classical physics while also laying the foundation for a potential interpretation of an otherwise uninterpreted fact of classical physics.

In this sense, the development of general relativity can be understood as being primarily motivated by the desire to remove inertial frames of their privileged status: this, as we will see, is equally clear throughout the development of the theory. EP1, of course, does not achieve this on its own: rather than remove the privileged status of inertial frames it extends that privilege to uniformly accelerating frames of reference. However, the equivalence principle did have an important role to play in the process by which Einstein stripped inertial frames of their privileged status: EP1 expanded the group of admissible coordinate transformations so that inertial frames were no longer invariants of the admissible transformations of the theory.[184]

While EP1 promised to neatly solve the problem that Einstein discusses in his (1954) memoir, it would also raise a further problem. In his (1907), Einstein used EP1 to demonstrate that the speed of light must change in the presence of a gravitational field, contrary to the expectations of special relativity. At some point between 1907 and 1911 Einstein became convinced that the essential insight of the equivalence principle was correct and that he must, therefore, resolve this new tension between the special theory of relativity and the prediction derived with EP1 that in a relativistic theory of gravitation, the speed of light must be variable. This, I suggest, was the most significant aspect of the development of a relativistic theory of gravitation in this period. It is clear from Einstein's later publications that he retrospectively saw this as the main challenge of the period:

> This result [that light bends in a gravitational field] is not in agreement with the present theory of relativity, for it leads to a dependence of the velocity in vacuum on the gravitational potential. However, I have shown together with Mr. Grossman, that the theory of relativity can be generalized in such a way that it remains in agreement with the above mentioned equivalence hypothesis. (1913b, p.190)

> The Einstein-Grossman theory is more complicated than that of Nordström in that it violates the…principle of the constancy of the velocity of light and thereby necessitates a generalization of the theory of relativity. But, in return, it eliminates an epistemological

[184] I make this more precise in §4.3.2.2. See also Norton (1989b).

> weakness that hitherto attached to mechanics and that has long been felt by perspicacious epistemologists, especially by Ernst Mach. (1914b, p.292)

In both these passages Einstein was concerned with a violation of the constancy of the speed of light and suggests that the purpose of his (1913a) is to show that it is possible to see it as result that is consistent with the equivalence principle.

4.2.2. 1907-1912: The equivalence principle in Einstein's theory of static gravitational fields

As we have just seen, in 1907 Einstein derived a contradiction between Newton's theory of gravitation and relativistic kinematics. EP1 is consistent with Newtonian physics and was used to derive a contradiction between Newton's theory of gravitation and the theory of relativity. The 1907 and 1911 papers should be seen as Einstein's deriving this contradiction: he showed that Newtonian physics and relativistic kinematics jointly imply the variability of the speed of light (which would seem to contradict the light postulate). Einstein, then, needed to find a way to resolve this conceptual tension. This conceptual tension became apparent as a consequence of Einstein's deployment of EP1, as such it was this principle that Einstein saw as being ultimately responsible for the conceptual tension of the 1907 and 1911 papers. In this section I examine Einstein's two papers of 1912 in which he developed a theory of static gravitational fields: the equivalence principle in these papers is used to motivate Einstein's abandonment of the Newtonian idea that gravitational interaction is mediated by a gravitational field.[185]

The statement of the equivalence principle in this paper is similar to the statement of the principle that we saw in Einstein's (1907), but differs from it a significant fashion. Here, Einstein introduced it in the following form:

> Let the reference system K (coordinates x, y, z) be in a state of uniform acceleration in the direction of its x-coordinate…According to the equivalence hypothesis, such a system K is equivalent to a system at rest in which there is a mass-free static gravitational field of a specific kind. (1912a, pp.95-6)

The difference between this formulation of the principle and the 1907 version is that now Einstein explicitly stated that the gravitational field was "mass-free".[186] This statement, then, specifically introduces the idea that gravitational fields can be conceived of as separate

[185] In this respect I endorse the argument of (Norton, 1989a, §4).

[186] Einstein clarified in a footnote that we have to imagine the masses that generate the field to be situated at infinity, I do not think, though, that this has any impact on the following argument.

from the masses that cause them. Whereas in the papers of 1907 (and 1911) the equivalence principle was taken just as the claim that the laws of physics take the same form in both cases, here there is an additional claim that the systems are fully equivalent. That this is a novel—and, for some, troubling—claim is indicated by Laue's rejection of the idea in an earlier letter to Einstein:

> I do not believe in this theory because I cannot concede the full equivalence of your systems *K* and *K'*. After all, a body causing gravitation must be present for the gravitational field in a system *K*, but not for an accelerated system *K'*. Thus, the presence or, else, absence of such a body will decide immediately whether we are dealing with a real gravitational field or only an accelerated system. (Max Laue to Albert Einstein, 27th December 1911, in Beck (1995, p.244))

The formulation of the equivalence principle that Einstein gave in his (1912a) includes, in effect, the claim that it is possible to conceive of fields as having existence independently of their source.[187] The equivalence principle of 1912, then, can be stated as follows:

(EP2): An accelerated frame of reference and a frame of reference equipped with a homogeneous gravitational field are *fully* physically equivalent.

The difference between EP1 and EP2 is as follows. According to EP1 the description of the physical situations is the same, however, there is still room to distinguish a "real" gravitational field from one generated by transforming away an acceleration. EP2, though, does not make this distinction. How does this alteration of the principle impact upon the development of the theory? Let us look at Einstein's (1912a), (1912b) and (1912c) to chart the development of his thought in this year.

The year in question, 1912, is important in the development of general relativity for a number of reasons: (1) Einstein's theory of static gravitational fields—which he took the first steps towards developing in his (1907) and (1911)—found its most developed form in his (1912a) and (1912b), (2) he moved to Zurich in August where he would meet up once again with Marcel Grossman, who would supply him with the mathematical tools to treat gravitation as space-time curvature and (3) in the winter of 1912 he began working towards his field equations in his Zurich notebook.

Prior to Einstein's work on developing a relativistic account of gravitation it had been assumed that gravitation was to be added to space-time structure as an additional

[187] Or, in this case, at least with their source removed to infinity.

field. In 1905 there was not expected to be any special problem in modifying Newton's gravitational theory to make it compatible with relativistic considerations. It was thought that it would be possible to develop a Lorentz-covariant version of Newton's expression for the gravitational force between two objects. Both Poincaré and Minkowski made serious attempts in this direction.[188]

The equivalence principle provided Einstein with an alternative to the field-theoretic approach. Rather than needing to find a way to render an additional structure consistent with space and time as given by relativistic kinematics, Einstein was able to claim that the space-time structures of special relativity are all that is needed to represent gravitation. The space-time of special relativity is all that is needed to deal with uniform accelerations; now that uniform accelerations are understood to be *fully* equivalent to the gravitational fields (as opposed to it being the case that only the equations of motion were the same in both cases) Einstein saw that the space-time structure of special relativity is all that is needed to account for gravitation as well.

Einstein (1912a) began by considering the observation of an accelerated frame of reference from the perspective of a non-accelerating frame with a constant gravitational potential and seeking coordinate transformations to describe the relation between the two frames. For an accelerated frame K (x, y, z, t) and inertial frame K' (ξ, η, ζ, τ) these are given by:

$$\xi = x + \frac{ac}{2}t^2$$
$$\eta = y$$
$$\zeta = z$$
$$\tau = ct$$

Where the velocity of light in system K is given by:

$$c = c_0 + ax$$

where c_0 and a are integration constants. Now, Einstein is in a position to appeal to a result of his (1911): that the gravitational field is determined by c (which is, of course, derived from the equivalence principle and is just the apparent contradiction between Newtonian physics and relativistic kinematics that we drew attention to in §4.3 considered from the

[188] See (Norton, 2007b, p.416).

reversed perspective). The description of the gravitational field is a solution to the following equation:

$$\Delta c = \frac{\partial^2 c}{\partial x^2} + \frac{\partial^2 c}{\partial y^2} + \frac{\partial^2 c}{\partial z^2} = 0$$

This equation, Einstein says (1912a, p.99), must be valid for every mass-free static gravitational field. The natural expression for the case with mass-sources would then be:

$$\Delta c = kc\rho$$

Where k is the universal gravitational constant and ρ represents mass density. In a footnote Einstein adds that these equations cannot be considered as entirely correct. The matter is left there in his (1912a) and is fully explored in his (1912b).

Einstein explains the problem with the previous equation in his (1912b, pp.114-5). The problem is, as he goes on to show, that this eventually results in an incompatibility with the principle of action and reaction. To show why this is so we also need the expression for the force $\mathfrak{F}$ that acts on a given density of stationary ponderable matter, ρ, that Einstein also derived in his (1912a):[189]

$$\mathfrak{F} = -\rho \mathrm{grad} c$$

Einstein then forms the integral:

$$\int \mathfrak{F} d\tau$$

If this is taken over a space for which c is constant at infinity, the principle of equality of action and reaction implies that it should vanish: "Otherwise the totality of the masses in the space under consideration…would strive to start moving" (1912b, p.115). That is, if the integral does not vanish then we would find that a uniform and infinite distribution of mass would set itself in motion. Now, combining the expression for force with expression for Δc we get:

[189] Einstein changes the notation for mass density from ρ to σ in his (1912b): I continue to use ρ for the sake of consistency.

$$\int \mathfrak{F} d\tau = -\int \rho \mathrm{grad} c \, d\tau = -\frac{1}{k}\int \frac{\Delta c}{c} \mathrm{grad} c \, d\tau$$

The last part of this equality does not vanish, though, which places the expression for the gravitational field—derived from the equivalence principle—in opposition to the principle of action and reaction.

Einstein tried two ways to avoid this incompatibility, but neither proved to be satisfactory.[190] Having exhausted these possibilities, Einstein's only remaining alternative was to alter his expression for the gravitational force on a body. His final equation was given by (1912b, p.118):

$$\Delta c = k\left\{c\rho + \frac{1}{2k}\frac{\mathrm{grad}^2 c}{c}\right\}$$

The second term here can be seen as naturally representing the energy-density of the gravitational field.

It is this term that ensures that Einstein's theory conserves energy. The difference between this and the (1912a) expression for the gravitational force on a body is that, now, the gravitational field itself acts as a source of gravity rather than just the mass-energy of matter acting as a source of gravitation. The reason that this secured energy-conservation was that, according to Einstein's famous equation $E=mc^2$, *all energy* should act as a source of gravitation (because matter and energy are of essentially the same type, so rather than matter causing gravity, energy does). This means that *this* expression and *not* the (1912a) one satisfies the demand of energy conservation, because it treats both the energy of matter and the energy of the gravitational field as a source of gravity. So, guided by the principle of action and reaction, Einstein had developed an expression for the gravitational force on a body that conserved energy.

However, this expression does not admit a linear dependence of the potential *c* on distance and so it cannot be used to describe homogeneous gravitational fields. This means that Einstein had a choice between accepting this expression for the gravitational force on a body and securing both the principle of the equality of action and reaction and the

[190] See Norton (1995, pp.44-5) for Einstein's reasoning. He first considered allowing gravitation to act on the bodies of the system because they were stressed and second tried inserting further terms into his expression for the gravitational force on a body: neither approach succeeded. Here I see Einstein as simply trying to retain the strongest possible form of the equivalence principle that he can; ultimately it loses out here to the equality of action and reaction and so is weakened. This, though, simply provided Einstein with the motivation to alter his understanding of the principle of equivalence again in the development of the final version of general relativity.

conservation of energy, or insisting on the equivalence principle and finding some other equation.

Einstein ultimately thought that it was more important that his equation did justice to well-established physical principles than obey a heuristic principle like EP2. As such, the only way to maintain a version of the principle of equivalence is to limit it to infinitesimal regions of inhomogeneous gravitational fields. Einstein's dissatisfaction with this is clearly expressed in his (1912c):

> I sought to make a first, quite modest contribution to the attainment of this goal [a theory of relativity that includes the equality of gravitational and inertial mass] in my papers on the static gravitational field…I have to admit that I was able to carry through this conception in a consistent way only for infinitely small spaces, and that I cannot give a satisfactory explanation for that fact. But I do not see this as any reason to reject the equivalence principle for the infinitely small as well; no one can deny that this principle is a natural extrapolation of one of the most general empirical laws of physics.[[191]] On the other hand, the equivalence principle opens up for us the interesting perspective according to which the equations of a relativity theory that would also include gravitation may also be invariant with respect to acceleration (and rotation) transformations. (1912c, p.133)

Here Einstein explicitly stated that he regarded his task as finding a relativistic expression of the equivalence principle. He regards the theory of static fields as only partially successful in that the equivalence principle only found expression in an infinitesimal form. However, Einstein thought that there was still promise for the principle in the sense that it suggests a more general invariance group—incorporating accelerations and rotations—for a relativity theory that includes gravitation.

In this section we have seen that the principle of equivalence played a vital role in Einstein's arguments of 1912 in that it permitted him to view gravitational effects as just part of the space-time structure of special relativity. However his attempts to derive satisfactory equations governing gravitation thus-understood are frustrated by a contradiction between the demands of the principle of energy conservation and the demands of the equivalence principle. Ultimately, for the time being, the demands of the equivalence principle are lessened to avoid this problem. However, the principle still looms large in his thoughts and, convinced that the principle is a "natural extrapolation" of empirical laws, Einstein hints that the principle may motivate widening the invariance

[191] Note that Einstein's understanding of the equivalence principle here seems slightly different to the understanding advocated by Howard. The empirical generalisation is just that masses fall in a gravitational field at the same rate regardless of their constitution. Einstein understands the equivalence principle as going beyond that: it is the "natural extrapolation" of this empirical fact.

group of relativity to include accelerations and rotations. At this point though, Einstein is stuck:[192] it is not until he is fortuitously reunited with Marcel Grossman that he will develop the tools to take the next step.

This is important from the perspective of understanding how Einstein would arrive at the version of the equivalence principle that eventually finds expression in general relativity:

> (EP3): Inertia and gravitation are entirely identical in nature and one structure—the inertio-gravitational field—is responsible for both.

In §4.3 I show how Einstein arrived at EP3 via the rotating disk thought experiment.

4.2.3. The equivalence principle as a regulative demand

In the previous two sections, I have detailed the introduction of the equivalence principle and shown how the principle developed in response to demands of the emerging general theory of relativity. In introducing EP1, Einstein did not go beyond Newtonian physics: the principle was used to draw attention to an inconsistency between Newtonian physics and relativistic kinematics in requiring the speed of light to be variable. The conceptual tension is resolved by strengthening the equivalence principle to the claim that uniformly accelerating frames of reference and inertial frames of reference equipped with a homogeneous gravitational field are fully physically equivalent: this meant that gravitational effects could be seen as arising from the structure of space-time. It is important to stress that in the 1912 theory there is not a full unification of gravitational and inertial effects: the equivalence principle remained the limited claim that a class of accelerated frames is equivalent to a class of inertial frames that are equipped with homogeneous gravitational fields. This was enough to motivate a move away from field-theoretic approaches to gravitation but ultimately forced Einstein to treat EP2 as holding only infinitesimally. Let us now turn our attention to the question of how we should philosophically interpret the process by which Einstein introduced the equivalence principle.

Friedman argues that Einstein, in introducing the equivalence principle, sought to emulate Poincaréan methodology in elevating an empirical fact to the status of a principle. Whichever version of the equivalence principle Friedman has in mind, it should now be

[192] Indeed, after the section of his (1912c) quoted above Einstein continues: "One can already see from the previously treated, highly specialized case of rest masses, that the space-time coordinates will lose their simply physical meaning, and it is not yet possible to foretell the form that the general space-time equations can have. I would like to ask all of my colleagues to have a try at this important problem!" (p. 133).

clear that this does not fit the historical narrative developed in §§4.2.1-2. EP1 is a pre-existing principle taken from Newtonian mechanics that Einstein used to identify the source of a conceptual tension between special relativity and Newton's theory of gravitation. EP2 is developed from EP1: EP1 was understood to be problematic because it would still be possible to distinguish accelerating from gravitational systems on the grounds that a body must be present in order to generate a gravitational field. EP2 re-emphasises the full equivalence of accelerating systems and those equipped with a homogeneous gravitational field. EP2, in effect, embraces in full the consequences of EP1 and seeks to treat gravitation using the space-time structures used to provide a relativistic account of gravitation. EP3, as we see more fully in §4.3.1, emerges from the application of an infinitesimal version of EP2 to the case of uniform rotation.

The important point is that the steps of this process that we have seen so far do not depend in any way on Einstein's seeking to implement Poincaréan methodology. Einstein's concern was with the compatibility of Newton's account of gravitation and his own theory of relativity. In his first attempt to unite these theories he discovered that the universality of free-fall was threatened. Rather than elevating the empirically observed fact of the universality of free-fall to the status of a principle, Einstein adopted a pre-existing classical understanding of the behaviour of inertial frames of reference and used this to guide the development of his relativistic account of gravitation so that there would be no further clash between the two theories. That is, there is no need to posit the elevation of a fact to the status of a principle because EP1 codifies the Newtonian understanding of the relationship between inertial and gravitational frames.

How, then, should we understand the motivation behind the equivalence principle? I suggest that it be understood as the implementation of a regulative demand to broaden the class of admissible coordinate transformations. In the period we have considered in this section, the equivalence principle certainly seems to act as a covariance principle. Prior to his work in 1907, Einstein treated all inertial coordinate systems as physically equivalent: i.e., special relativity was Lorentz covariant. In his work between 1907-1912 the equivalence principle is appealed to in order to expand the covariance group of special relativity so as to also include uniformly accelerating coordinate systems. To be sure, this does not extend the covariance group of relativity as far as general covariance eventually would do; however, the 1912 theory of static gravitational fields does represent an intermediate stage in the development of general relativity with an intermediate set of physically equivalent coordinate systems.

There is another important lesson to be drawn from this period of the development of general relativity. In §3.3.2.2, I suggested that on a regulative Kantian philosophy of science, constitutive principles should be understood as those that played a role in making the new laws of a theory possible. The equivalence principle certainly seems to have played this role in 1907-1912. Cassirer argued that for physical principles to play this role they must be "universal", i.e., they must find some form of expression in both successor and precursor theories so that they can point the way from the old theory to the new theory. The equivalence principle is universal in this sense, because it finds expression in Newton use of Corollary VI and Galileo's equivalence principle to permit the treatment of the system of Jupiter and its moons as an inertial system.[193] However, the principle should not be expected to retain the same form throughout theory change and may need to be amended in order to bring new laws under it: I have argued that, through a process of dialectical engagement, the equivalence principle develops in just such a way so as to enable the lawlike description of an ever-widening class of phenomena.[194]

Now, this might raise a worry. Cassirer's account of the role of principles in the development of new physical laws may seem very close to Poincaré's account of elevation: Cassirer, after all, takes physical principles to be hypotheses that are held to be "universal" in order to develop new laws (DIMP, pp.53-4). Does the above, then, not go some way towards vindicating Friedman's account of the development of relativity? I think not. It bears emphasising again that Friedman's answer to CR requires that Einstein *self-consciously* implemented Poincaréan methodology.[195] I do not see any evidence that this is what Einstein intended. Instead, his motivation seems to have been to unify Newton's account of gravitation with the special theory of relativity. EP1 was not "elevated", it was a hypothesis of Newton's theory that Einstein saw was able to serve the twin purpose of (1) enshrining the universality of free-fall and (2) beginning the process of gradually stripping away the privileged status of inertial frames of reference which Einstein saw to be the main

193 Here there is a parallel to be drawn between my account and Post's (1971) account of the development of new scientific theories. Post seeks to show that heuristics play a crucial role in the development of scientific theories: he identifies eight heuristic criteria for the rational development of new scientific theories. There are two criteria that Post identifies that are particularly relevant to my account: the "footprint" and the "general correspondence principle", which I discuss in §4.3.2.2. In general relativity, Post argues, general relativity has a footprint in classical physics in the sense of the equality of gravitational and inertial mass in classical physics. The two concepts of mass needed to be separated in order to give meaning to Newton's laws of motion, but—as we have seen in §4.2.1—the separation is undone in Newton's treatment of gravitation. In this sense we can treat EP1 as the expression of general relativity's footprint in classical physics.

194 Cf. (Cassirer, DIMP, pp.53-7)

195 For the development of a new theory to be rational, on Friedman's account, scientists must knowingly appeal to philosophy in order to ground their development of new concepts. This is important for two reasons. First—especially in the case of the type of meta-scientific philosophical discourse that Friedman is primarily concerned with—it ensures that the individual scientist or group of scientists that are developing a new theory appeal to concepts that are consistent with scientific knowledge up to that point. Scientists, of course, may also make original philosophical contributions themselves, but I would speculate that on Friedman's account these original contributions must be concerned with the same problems that characterize the philosophical meta-paradigm. Second, engagement with philosophy ensures that there is a paradigm-neutral framework which can be appealed to in order to convince other scientists of the rationality of the new theory.

obstacle towards successfully unifying the two theories. The rationality of this process lies in Einstein's identifying a conceptual tension between the two theories he sought to unify and methodically seeking to resolve it. This requires no appeal to a philosophical meta-paradigm to be seen as rational either retrospectively or prospectively: the desire for unity and the process of conceptual analysis are sufficient to jointly ensure the rationality of the development of the new theory.

A regulative answer to CR, then, is well-placed to account for this aspect of the development and role of the equivalence principle in the emerging theory of general relativity. Regulative answers to CR, recall, need to show first that each theory in the historical sequence of, in this case, space-time theories deals with the same subject matter. Einstein's introduction of EP1 as a principle taken from Newtonian physics offers a way to secure this: i.e., if general relativity is understood as emerging from a desire to correct a conceptual ambiguity in Newtonian mechanics, then there is a clear sense in which it represents a continuous development from Newtonian physics rather than an entirely new theory. Second, the process was begun by Einstein's desire to unify two theories: the demand for unity is the paradigmatic Kantian regulative demand. Finally, CR must provide an account of why successor theories are to be preferred to the earlier theories. An important aspect of the regulative answer is that the successor theory represents a better approximation of objectivity in the sense that it endorses a broader range of coordinate transformations. As we have seen, there is a clear sense in which both EP1 and EP2 should be understood as motivating precisely such an increase in the number of admissible transformations. These are the foundational parts of a regulative answer to CR: I will show in the rest of the chapter how they can be applied to explain the rationality of the emergence of Einstein's final (1916) statement of general relativity.

4.3. The role of the rotating disk thought experiment in the development of general relativity

By March 1912, Einstein had completed his work on static gravitational fields and immediately turned his attention towards considering stationary gravitational fields. The natural place to begin such a study was with the gravitational field that would be generated by a rotating disk.[196] Recent scholarship[197] emphasises the importance of the rotating disk

[196] It is clear from Einstein's correspondence that at an early stage of his investigations into stationary gravitational fields he understood the case of rotation to be important. For example, in a letter to Ehrenfest he clearly distinguishes between the static case and that represented by a rotating ring: "I am sending you my papers on gravitation, the latest of which you do not have. According to it, it appears that the equivalence principle can be valid *only for infinitely small* fields, and that, therefore, Born's accelerated *finite* system cannot be

thought experiment as a "missing link" in the development of general relativity: the thought experiment is understood to have been the primary motivation for Einstein's coming to represent the gravitational field as curvature of four-dimensional space-time. The rotating disk also plays a crucial role in allowing Einstein to formulate the equivalence principle as EP3. As we have, seen, early in 1912 Einstein had been forced to adopt an infinitesimal version of EP2: as will we see in §4.4, when he began his search for his field equations in the winter of 1912 Einstein understands the equivalence principle as the claim that inertial and gravitational effects are both attributable to the inertio-gravitational field.

As detailed in §1, Friedman—in both his (2001) and (2010a)—places great emphasis on the rotating disk thought experiment: it plays a role in his answers to both CR and CC. First, for Friedman, the rotating disk serves to answer CR by providing a concrete example of philosophy playing a crucial role in the conceptual development of a scientific theory.[198] Second, the rotating disk serves to ground the constitutive role of the equivalence principle: it is through the equivalence principle's role in the rotating disk thought experiment that Friedman argues that variably curved four-dimensional space-time became a genuine physical possibility rather than a merely mathematical possibility.

In this section I clarify the sense in which the rotating disk helped Einstein to the realisation that gravitation could be represented by space-time curvature, given by the metric. Friedman's account emphasises the role that measuring rods and clocks played in the thought experiment: he argues that it was through considering the behaviour of rods and clocks in a rotating frame that Einstein came to see that he could treat gravitation as curvature of four-dimensional space-time. In broad detail, Friedman's account is right: the rotating disk does seem to have influenced the introduction of the metric as a measure of space-time curvature. However, I suggest the manner in which the thought experiment proceeds means that (i) there is no need to read Einstein as engaging in a delicate dance between the philosophies of Helmholtz and Poincaré and (ii) it is not plausible to understand the rotating disk as grounding the constitutivity of the equivalence principle. I argue, instead, that the rotating disk plays a regulative role in the same way that we saw EP1 and EP2 did in the early stages of the theory: i.e., it broadened the range of admissible coordinate transformations.

considered a static gravitational field, i.e., cannot be generated by masses at rest. A rotating ring does not generate a static field in this sense, even though it is a temporally invariant field." (Einstein to Paul Ehrenfest, before 20 June 1912, CPAE 5, p.310, underlining mine). See (Stachel, 1989a; 2007) for a detailed account of the importance of the rotating disk thought experiment in Einstein's development of general relativity between March and July 1912.

197 Most notably, (Stachel, 1989a) and more recently (Janssen, 2005)

198 See §1.3.3: Friedman argues that in the thought experiment Einstein situates his view of geometry between Poincaré and Helmholtz's epistemologies of geometry.

4.3.1. *How did Einstein come to represent gravitation as space-time curvature?*

The first question to address is as to how Einstein understood the role of the rotating disk thought experiment: I suggest that Einstein primarily saw the rotating disk as impacting on his understanding of coordinate systems. Einstein's memoirs repeatedly state that freeing himself from the preconception that coordinate systems have an immediate physical significance was the main problem that he faced in developing general relativity. In his *Autobiographical Notes*, Einstein asks why it took him so long—seven years from 1908—to arrive at the final theory of relativity and he answers as follows:

> The main reason lies in the fact that one does not free oneself so easily from the conception that an immediate physical significance must be attributed to the coordinates. (1996 [1949], p.67)

Einstein gave a more precise answer in his (1954 [1933]):

> I soon saw that, according to the point of view about non-linear transformations required by the equivalence principle, the simple physical interpretation of the coordinates had to be abandoned; i.e., one could no longer require that coordinate differences be interpreted as signifying the results of measurements with ideal measuring rods and clocks. The recognition tormented me a great deal because for a long time I was not able to see just what *are* coordinates actually supposed to mean in physics? The resolution of this dilemma was reached around 1912. (1954, p.288)

Now, while these are both quotes from memoirs that Einstein wrote a significant amount of time after the period of the development of relativity in question, they do—I suggest—provide insight into the particular problem that most troubled Einstein in 1912. In this section I argue that the introduction of the four-dimensional metric as the measure of space-time curvature was a response to this trouble with how to understand coordinate systems. However, this—as is suggested by Einstein's (1949) claim that it took seven years to free himself from the idea that coordinate systems have an immediate physical significance—was not the end of his struggle to understand coordinate systems.

In developing special relativity, Einstein had understood coordinate differences as having direct physical significance in the sense that he took them to correspond to measurements made by rigid rods. This is quite clear in the very first section of his (1905):

> The theory to be developed—like every other electrodynamics—rests upon kinematics of rigid bodies, since the assertions of each theory concern relations between rigid bodies (coordinate systems), clocks and electromagnetic processes. (1905, CPAE 2, p.277)

Here Einstein seems to go so far as to take "rigid bodies" and "coordinate systems" to be synonymous. It quickly became clear to Einstein, though, that this understanding of how coordinates should be understood—in terms of rods and clocks—was not tenable. Einstein began his (1907) paper with an argument intended to show that coordinate differences in a uniformly accelerating frame of reference correspond—to first order approximation—to measurements with rigid rods: this enabled him to apply EP1 to show that coordinate differences retain this direct physical significance in homogeneous gravitational fields. That Einstein felt the need to rehearse this argument suggests that even as early as 1907 he had begun to suspect that his (1905) understanding of coordinate differences might need to be revised when he came to deal with more complex gravitational fields. Furthermore, he went on to show that the local time measured by clocks must differ from the universal time that he took to be necessary to define the simultaneity of distant events.[199, 200]

This difficulty was compounded by Einstein's realisation in early 1910 at the latest, that rigid bodies are incompatible with the theory of relativity. In a letter to Sommerfeld, Einstein wrote:

> And now to the other problem child, the rigid body. I occupy myself very little with it. For it seems to me that the empirical data do not suffice for the construction of a theory of arbitrarily accelerated bodies. Had it not been for Fizeau's experiment and the measurements concerning the velocity of light in vacuum, we would not have had the material need for the construction of the relativity theory; we are, in my opinion, in a similar situation with respect to acceleration. It is only about infinitely slowly accelerated systems that anything at all can be asserted at the moment, in my opinion. Nevertheless, one should try to devise hypotheses about the behaviour of rigid bodies that would allow a uniform rotation. (Einstein to Arnold Sommerfeld, 19 January 1910, CPAE 5, p.229).

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[199] Einstein repeated this more clearly argument in his (1911): here he used the difference in the rate at which clocks read off time to explain the variability of the speed of light. See (1911, CPAE 3, pp.383-5)

[200] As an aside, it is worth mentioning that Einstein's account of this aspect of his (1907) in his *Autobiographical Notes* (1949) is accurate. In his (1949) he wrote: "the time in which the [gravitational] field appears to be static is *not* measured by *equally constituted* stationary clocks. From this special example one can already recognize that the immediate metric significance of the coordinates is lost once one admits nonlinear transformations of the coordinates" (p.63 & p.65). So, here Einstein suggests that as long as we perform only linear transformations, then the immediate physical significance of coordinate differences can be maintained. As soon as we seek to go beyond such transformations, though, this approach is no longer tenable. This seems to be the very same argument as given in (1907)—albeit that in 1907 Einstein did not explicitly mention the potential problem for non-linear transformations—and this, I suggest, should increase our confidence in the accuracy of Einstein's 1949 diagnosis of the central problem that he faced in developing general relativity.

While Einstein states that he is not, in 1910, spending too much time worrying about how rigid bodies can be made consistent with the theory of relativity, he is clear that it would be helpful to try and understand rigid bodies in such a way that they could be understood as relativistically rotated. A couple of months later, in a letter to Jakob Laub, Einstein seems to accept that this task is hopeless:

> The latest relativity-theoretical investigations of Born and Herglotz interest me very much. It really seems that the theory of relativity there does not exist a "rigid" body with 6 degrees of freedom. (Einstein to Jakob Laub, 16 March 1910, p.232)

So, rigid bodies with 6 degrees of freedom are impossible in a relativistic setting, because if a rigid body rotates relativistically it will be deformed and, as such, is not a rigid body. Why is this a problem for Einstein's understanding of coordinate systems? The problem is that as soon as one accepts that rigid bodies are incompatible with the theory of relativity, then Einstein's favoured method of understanding coordinate differences is also impossible. Einstein's first published account of the problem posed by a rotating system is in his (1912a) and goes as follows:

> The spatial measurement of *K* is done with measuring rods that — when compared with each other at the same place in *K* — possess the same length; the theorems of geometry are assumed to hold for lengths measured in this way, and thus also for the relations between the coordinates x, y, z and other lengths. That this stipulation is allowed is not obvious; rather it contains physical assumptions that eventually could prove incorrect. For example, it is highly probable that they do not hold in a uniformly rotating system, in which, on account of the Lorentz contraction, the ratio of the circumference to the diameter, using our definition of lengths, must be different from π. (Einstein 1912a, CPAE4, Doc.3, p.96)

Here the problem is that if one accepts that the spatial measurement of coordinate differences within a frame is to be performed with measuring rods, then one would not measure Euclidean geometry in a rotating system. His conclusion is that such an understanding of coordinate differences is a physical assumption that may well, ultimately, prove false.

The (1912a) statement of a rotating frame thought experiment, then, is primarily important because it encapsulates Einstein's puzzlement about the relationship between measuring rods and clocks and coordinate systems. Einstein was, in all probability aware of this problem with a simple physical interpretation of coordinate differences as signifying measurements with ideal rods and clocks: the correspondence with Sommerfeld and Laub, above, suggests that Einstein was aware of this much by 1910. Stachel (1989a; 2007a) takes the rotating disk thought experiment to be such a crucial episode in the development of general relativity because it helped Einstein along to a new understanding of coordinate systems. How did it do this? More precisely, what was it about the case of rotation that led Einstein to adopt the Gaussian line element *ds* as the physically measurable quantity?

The first step was for Einstein to realise that, while rigid bodies are impossible in a relativistic theory that permits rotation, rigid *motions* remain perfectly possible.[201] Einstein would probably have been aware of this by 1911. Herglotz, whose work Einstein referred to in his 1910 letter to Jakob Laub, had argued in a paper of 1910 that when one of the points of a rigid body is fixed rigid rotation about that point is perfectly possible. Laue made a similar point in a paper of 1911:

> The limiting concept of a body that is rigid under all circumstances, which is so useful everywhere in classical mechanics, in my opinion cannot be taken over [to the special theory—JS] on account of the impossibility of indefinitely large velocities for the propagation of elastic deformation. However, this does not exclude a body moving at times like a rigid one; even according to classical mechanics, under certain circumstances a drop of fluid can move as if it were rigid. (Laue, 1911, p.107 translated in Stachel, 2007a, p.90)

So, by 1911 it was known that the problem of the impossibility of rigid bodies in relativity theory could be avoided by dealing only with the question of rigid motions—whereby a body moves as if it were rigid—and not concerning oneself with the more fundamental question as to whether rigid bodies really exist and as to how one should describe their behaviour under accelerations. Stachel (*ibid.*, p.90) characterises this as the transformation of a dynamical problem concerning the nature of objects into a kinematical problem about rigid motions. The kinematical problem could be solved much more easily.

This can be seen very clearly in the case of a rotating disk. The dynamical problem is to try and determine the *behaviour* of a rigid disk that undergoes a relativistic rotation: there is no obvious way to answer this question.[202] However, if the problem is understood

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[201] The importance of this realisation to the development of general relativity is emphasised in (Stachel, 2007a, pp.89-90)

[202] In fact, the dynamical problem continued to exercise philosophers and physicists after general relativity had been completed. Petzoldt, e.g., argued that if a disk was understood as a series of concentric rings then, under rotation, the ratio of the diameter of a disk to its circumference would be less than π. This is because, he argued, the diameter of a disk would remain constant while the disks would contract. However, as Einstein pointed out to Petzoldt in correspondence, this is mistaken because if a disk were a series of concentric rings, then the radius of the disk would contract under rotation with its circumference. Petzoldt's argument may

kinematically, its solution is quite simple. Instead of concerning ourselves with the behaviour of the disk itself, one asks instead how the geometry of the disk would be measured. This involves only rigid motions, because any rigid rod that is placed on the surface of the disk does not, itself, rotate. This is the idea that is important: while one cannot determine the precise behaviour of a rotating disk one can determine its surface geometry and this is non-Euclidean.

This is the point at which Friedman claimed that Einstein made appeal to Helmholtz's philosophy. On Friedman's account, Einstein came to see the geometry of the rotating disk as non-Euclidean through the application of Helmholtz's understanding of physical geometry. It should be clear from the above that no appeal to Helmholtz is necessary. Measurement of coordinate differences using rigid rods was common practice in nineteenth century physics:[203] in developing general relativity, Einstein found this methodology threatened by the incompatibility of rigid bodies and a theory of relativity that admits rotations. His solution to this problem was influenced by the physics community, in particular Einstein took from Herglotz the idea that while rigid bodies are incompatible with special relativity, rigid motions are not. This allowed Einstein to describe the surface geometry of a rotating disk by the familiar methodology of taking coordinate differences to correspond to measurements with rigid bodies. No appeal to Helmholtz's philosophy is necessary here: it is a problem derived from physics and its solution is likewise derived from physics.[204]

At this stage, then, Einstein had the conceptual tools available to understand the rotating disk in the manner that he set out in 'Geometry and Experience'. Measuring rods can be used to determine the geometry of the surface of a rotating disk: because these rods contract when placed along the circumference of the disk and are not affected when placed

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still, though, suffice to show that the geometry of a disk is unchanged by relativistic rotation: this was the understanding of relativistically rotating rigid bodies that Eddington argued for. See (Stachel, 1989a) for a detailed of alternative interpretations of the rotating disks.

[203] Indeed, in §3.3.1 we saw Hertz advocate precisely this understanding of measurement.

[204] There is a related problem, as we will see in the discussion of 'Geometry and Experience' in §4.3.2, about how the line element *ds* gains its physical significance. In 'Geometry and Experience' Einstein seems to suggest that the line element is only physically meaningful because it can be determined by measurements with rods and clocks. This, I take it, is a slightly different question than the one at stake here. In this section I have argued that Einstein realised that the line element must be used to measure space-time curvature because it is frame-invariant and, as such, is a physically meaningful quantity. The question addressed in 'Geometry and Experience' as to *why* the line element is physically meaningful—i.e. is meaningful because it corresponds to measurements with rigid bodies and clocks or because it is frame-invariant—comes after Einstein has realised that it must be appealed to on account of its frame-invariance. For Friedman's account of the role of the thought experiment to work the philosophical question as to how a geometry is made physical must be *prior* to the development of the development of four-dimensional space-time as a physical possibility. However, I would argue that the philosophical question as to why the line element is physically meaningful only becomes a matter for concern *after* the frame-invariance of the line-element has been appealed to in order arrive at the idea that gravitation should be represented by variably curved four-dimensional space-time.

radially, the geometry of the disk will be measured as non-Euclidean. Similarly, clocks placed on the surface of the disk will read off time more slowly the further away from the centre of the disk that they are placed. Friedman (2010a) suggests that, from this point, it was a relatively straightforward matter for Einstein to see that gravitation should be understood as curvature of four-dimensional space-time, given by the line element. The first step is to apply the equivalence principle—at this stage the infinitesimal version of EP2—so that the inertial forces caused by rotation can be treated as gravitational forces. From this point Friedman argues that the development of the representation of gravitation as space-time curvature followed quite quickly:[205]

> It was at precisely this point, as Stachel shows, that Einstein first realized that gravitation could be represented by a non-Euclidean geometry, and, since an analogous effect holds for the temporal coordinate of the non-inertial frame in question (by time-dilation), he quickly realized that a non-Euclidean generalization of the flat (pseudo-Euclidean) metric of Minkowski space-time was exactly what he needed. The idea that the action of gravitation could be represented by a variably curved four-dimensional geometry—by the (metric) affine connection in a perturbation of Minkowski space-time—was finally in place. (2010a, p.663).

Friedman offers the following, more detailed account of this process:

> [The] crucial factor in motivating a truly four-dimensional understanding of the principle of equivalence is that the non-inertial (uniformly) rotating frame in question has not only a non-Euclidean spatial geometry (due to length contraction) but also a non-standard temporal metric (due to time dilation)…rigorously defining the non-Euclidean spatial geometry in this case is actually rather delicate, requiring, in effect, the consideration of all the instantaneous inertial systems that are tangent to the non-inertial frame at every point. The situation is similarly delicate with the non-standard temporal metric in question, where, once again, the result is rigorously well defined only if the frame is rotating *uniformly*. (Friedman 2010a, p.789n303)

As we have seen, up until this point in the development of general relativity, Einstein had been working in three dimensions: the problem with the rotating disk thought experiment is that it cannot be straightforwardly analysed in three-dimensional terms. The reason for

[205] See (Friedman, 2010a, p.663) for the other argument. The gist of the argument

this can be expressed most clearly by making use of Norton's (1989a, §3) notion of a relative space.[206]

To clarify this concept it will be helpful to explain how coordinate systems are currently understood in general relativity. The modern understanding of space-time theories posits a differentiable manifold *M*, which represents space-times and upon which various geometric object-fields, O_1, O_2 etc., are defined. The manifold and the fields defined upon it determine the set of a particular theory's models. So, special relativity has models of the form $\langle M, g_{ab} \rangle$ where *M* is a four-dimensional manifold and g_{ab} is the Lorentz metric field; general relativity has models of the form $\langle M, g_{ab}, T_{ab} \rangle$, where T_{ab}, additionally, is the stress-energy tensor which is such that it satisfies Einstein's field equation. A frame of reference is understood as a congruence of time-like curves defined on $\langle M, g_{ab} \rangle$. Each event of the manifold has a world-line associated with it, and these world-lines together define a frame of reference. The frame, then, via its tangent vector also assigns a velocity to each event.[207]

With each frame of reference there is an associated three-dimensional manifold, which is given by the set of the curves of the frame: i.e., if we take the time-like curves to be world-lines of physical bodies then the three-dimensional manifold would be defined by these physical bodies. This three-dimensional manifold is the relative space that is associated with a particular frame of reference.[208] The geometrical properties of a relative space can be determined by measurements with infinitesimal rigid rods: this means that we can profitably understand Einstein—prior to fully developing the four-dimensional formalism for general relativity—as working within relative spaces.

Within a relative space it is sometimes possible to define a frame time. Consider a set of world-lines of physical bodies: it is possible to divide the manifold up into various hypersurfaces that are, in effect, various instantaneous "snapshots" of the relative frame from a variety of different perspectives. We can define a frame time within a relative space when the hypersurfaces are all orthogonal to the curves of the relative space in the relevant region of the frame. When this is the case, any curve that is in this way parameterised by the hypersurfaces can act as a clock that measures time within the relative space.

We are now in a position to see why Einstein would have seen that, working within the three dimensional framework, there would have been a non-standard temporal metric in the case of a rotating disk. A rotating frame does not have orthogonal hypersurfaces and,

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206 Here I follow Norton's account, see his (1989a) for detail.

207 We can also then say that a coordinate system $\{x^i\}$ ($i = 1, 2, 3, 4$) is "adapted" to the frame of reference if it is the case that the spatial coordinates—curves of constant x^1, x^2 and x^3—are curves found in the frame.

208 For a formal definition of a relative space see (Norton, 1989a, pp.11-2)

as such, no frame time can be straightforwardly defined. This is why Friedman states that defining the non-Euclidean geometry requires "the consideration of all the instantaneous inertial systems that are tangent to the non-inertial frame at every point": there is no simple way to describe the evolution of the system without considering the sum of all instantaneous inertial systems. This requires four-dimensional mathematical methods and, thus, required Einstein to move beyond consideration of three-dimensional relative spaces.

How did Einstein come to realise that he needed to use a four-dimensional generalisation of Gauss's theory of surfaces? Simply, non-Euclidean spatial geometries cannot be described using standard Cartesian coordinates; they must be described using Gaussian coordinates. This, combined with the breakdown of relative spaces in the case of rotations, means that a four-dimensional variably curved space-time would have seemed the most natural way to describe a rotating disk.

This would mean describing space-time as a metric defined on a four-dimensional manifold. The curvature of such a space-time is given by the metric, g_{ik}. In the case of the rotating disk, space-time curvature could be understood either as an inertial effects or as a gravitational effect: the natural way to interpret g_{ik} physically, then, was as a combined inertio-gravitational field. This was how Einstein got to (EP3) as expressed in his (1913):

(EP3): Inertia and gravitation are entirely identical in nature and one structure—the inertio-gravitational field—is responsible for both.

Here it is worth noting a further distinction between my account and Friedman's. For Friedman, EP3 is constitutive of general relativity on account of the role it played in the development of general relativity. On my account it is an infinitesimal version of EP2 that is applied to the rotating disk thought experiment and this gives rise to EP3 as the natural way to interpret the equivalence principle.

Einstein was now in a position to begin the search for his field equations in the Zurich notebook. He had a starting point of three basic elements: (i) his 1912 scalar theory of gravitation, (ii) the four-dimensional formalism for special relativity and (iii) the representation of gravitation by curvature of four-dimensional space-tine.[209] I do not intend to go into the process by which Einstein derived his field equations, as the matter is too complex to do justice to in a limited space and has been dealt with in great detail by Janssen *et al.* (2007). I have detailed the process by which Einstein arrived at each of these elements and highlighted the role of the evolving equivalence principle in this process. Let us now

[209] See the commentary to the notebook (Janssen *et al.*, 2007, §2.1)

turn our attention to the question of how we should interpret the roles of the rotating disk and the equivalence principle in the development of general relativity.

4.3.2. How does the rotating disk help answer the challenges of rationality and constitutivity?

Friedman's claim that the rotating disk played a crucial role in the process by which Einstein came to treat gravitation as curvature of four-dimensional space-time is perfectly justified. However, Friedman makes two claims about the rotating disk thought experiment that I do not think can be sustained in light of the above historical analysis. First, he argues that the thought experiment can only be fully understood by situating it in the context of a philosophical dispute between Helmholtz and Poincaré. I suggest that it can be perfectly well understood without appeal to philosophy: Einstein considers rotation because it is the next natural extension of the class of admissible coordinate transformations and he assigns measuring rods their particular role on the ground of physical considerations. Second, Friedman claims that the thought experiment is important because it makes four-dimensional space-time a genuine physical possibility: the equivalence principle played a crucial role in the thought experiment[210] and it is for this reason that it can be understood as constitutive of general relativity. In §4.3.2.2 I clarify the precise sense in which the rotating disk should be understood as having made four-dimensional space-time: I argue that the equivalence principle, at this stage of general relativity, should be understood as a regulative principle.

4.3.2.1. The rotating disk: a role for philosophy in theory change?

The rotating disk thought experiment is—with the claim that Einstein was motivated by Poincaré's conventionalist methodology—the basis of Friedman's claim that philosophy played a crucial role in the development of general relativity. The discussion of the previous section should make it clear that it is not necessary to appeal to Helmholtz's understanding of geometry in order to explain Einstein's use of measuring rods to measure the surface geometry of a rotating disk. Throughout the development of relativity Einstein was struggling to work out how he should understand coordinate systems. It was clear to him from a relatively early stage—around 1910—that coordinate differences could not have direct physical significance in the sense that they correspond to measurements with rigid

[210] The discussion of the previous section has clarified the role of the equivalence principle in the thought experiment: it played an important role because it meant that a non-Euclidean spatial geometry was an effect of gravitation as well as an effect of inertial forces and it was this that led Einstein to treat g_{ik} as representing the inertio-gravitational field.

measuring rods; rigid bodies are incompatible with special relativity. While it seemed impossible to provide an account of rigid bodies that was consistent with special relativity, the question as to which types of rigid *motion* are possible proved to be much more tractable. It was this step that permitted Einstein to describe the surface geometry of a rotating disk.

Friedman's claim that the scientific philosophy of Helmholtz and Poincaré played a crucial role in the thought experiment is derived from Einstein's account in 'Geometry and Experience'. As we saw in §1.3.3, Einstein's lecture does provide strong support for this claim of Friedman's: if the account of the previous section is to be compelling, then, we must provide some other explanation for why Einstein did place such emphasis on the work of Helmholtz and Poincaré. I argue that the real target of 'Geometry and Experience' is Weyl's account of the role of the line element in general relativity.[211] Einstein delivered the lecture at the Berlin Academy's Leibniz-day celebration on 27 January 1921. In the years prior to this Weyl and Reichenbach had been engaged in a dispute over the epistemological status of measuring rods and clocks in the theory of relativity. Weyl understood rods and clocks as explananda to be explained by the total field; Reichenbach understood rods and clocks to be epistemological primitives that were required to make physical sense of mathematical geometries.[212] I suggest that Helmholtz and Poincaré were referred to in this lecture primarily because Einstein saw their dispute over the epistemic status of measuring rods as anticipating certain features of the debate that Einstein was actively involved in.

Einstein's formulation of general relativity is in terms of a semi-Riemannian manifold. Weyl was deeply unsatisfied by Riemannian geometry and sought to apply the techniques of the pure infinitesimal geometry—pioneered by Levi Civita in 1916—to Einstein's theory. He thought that a pure infinitesimal geometry was capable of correcting a "blemish" in Riemann's geometry. This blemish arises from Riemann's asymmetrical treatment of the magnitude and orientation of vectors. The problem is that when one transports, in infinitesimal steps, a vector around a closed path, when it returns to its starting position its orientation will have changed, but its length will not have. The direct consequence of this is that it is only possible to compare lengths of a vector that are separated by an arbitrary distance: one cannot so compare orientations. Weyl sought to develop a pure infinitesimal geometry that allowed for the comparison of the orientation of vectors across an arbitrary distance.

[211] Ryckman (2005) also emphasises that the Reichenbach-Weyl dispute forms the proper context of Einstein's lecture.

[212] See (Ryckman, 2005) for a detailed account of Weyl's work on relativity.

This is achieved by comparing neither orientation nor length directly: in both cases the comparison takes place by parallel transport of a comparison vector in infinitesimal steps along the path between the two vectors we wish to compare. With the Riemannian account so-adjusted, Weyl shows that it now has a much weaker, conformal—i.e., angle-preserving— structure. The concepts of conformal (and projective) geometries are important, because it is these that Weyl thinks bestows upon geometry its physical significance: it is worth taking a moment to clarify what conformal and projective geometries are and why Weyl takes them to be physically significant.[213]

Projective and conformal geometry are both arrived at by means of abstraction from affine geometry as described above. The conformal character of a space is captured by the claim that each point is associated with an infinitesimal cone of null directions, i.e.:

$$g_{ik}dx_i dx_k = 0$$

The conformal character of space is preserved when the metric of the space changes while leaving the cone at each point of space unchanged: this can generally be achieved by arbitrary linear metric transformations. An affinely connected space has a projective character if there is a parallel displacement that acts on an arbitrary direction at an arbitrary point p when p is infinitesimally displaced in that direction. I.e., if it is the case that if a point is displaced it is possible to project parallel displacements onto the space. Weyl claimed that projective and conformal characters of space have direct physical significance:

> In relativity theory, the projective and conformal characters have an immediate intuitive meaning. The former, the persistence of the world-direction of a moving particle, which singles out a certain "natural" motion when it is released from a particular point, is a unification of inertia and gravitation that Einstein posed in place of either notion, for which, however, no suggestive name has emerged, as of yet. The infinitesimal cone, however, describes the difference between past and future in the neighborhood of a world-point; the conformal character is the cause-and-effect structure of the universe, through which one may determine which world-points can possibly be causally connected to each other. (Weyl, 1921, p.2)

Projective properties of space, then, are physically instantiated by the claim that force-free particles follow space-time geodesics. I will return to this point in §4.4, but it is worth noting in passing that Weyl takes the equivalence principle to amount to this—geodetic—hypothesis. Weyl talks of the infinitesimal cones that define the conformal character of

[213] My account here follows Weyl's (1921).

space as describing the distance between past and future. It is clear, then, that the infinitesimal cones are interpreted as infinitesimal *light* cones: as such they fix the causal structure of space-time. This means that Weyl is able to claim that the "*projective and conformal character of a metric space determine that metric uniquely*" (*ibid.*): i.e., the space-time metric can be fully determined without appeal to rods and clocks.

This was in stark contrast to the epistemology of Reichenbach. Reichenbach followed Schlick in drawing a sharp distinction between mathematics and physics: mathematics was built upon a system of implicit definitions that, in order to be physically meaningful, needed to be coordinated with concrete physical objects.[214] Reichenbach and Weyl began to correspond on this difference of opinion in 1920 and would continue doing so until 1925.[215] I suggest that 'Geometry and Experience' should be read as Einstein weighing into this dispute on the side of Reichenbach: further, I think it is plausible that the philosophical concerns that faced Einstein in 1921 may have impacted on the accuracy of his recollections on the role that Helmholtz played in the development of general relativity.

This reading is supported by the fact that Einstein seems to refer to his pre-history objection to Weyl's theory in 'Geometry and Experience'.[216] Friedman takes 'Geometry and Experience' to be a defence of the view that space-time curvature is empirically determinable, against the conventionalism of Poincaré. It is not quite clear, though, why Einstein would feel the need to defend the view that the geometry of space-time is empirically determinable in the immediate aftermath of Eddington's 1919 expedition's empirical confirmation of the theory. The most likely explanation is that Einstein may have felt as though there was a contemporary objection to his theory that was Poincaréan in spirit.[217] Given that Reichenbach and Weyl were engaged in a dispute about the status of measuring rods in relativity, Weyl's account of rigid rods is the most natural candidate.

Einstein's defence of the use of rigid rods as grounding physical geometry, makes good sense when read as a response to Weyl. Einstein claims that generally relativity rests upon the idea that the Riemannian metric can be connected to experience only by consideration of rigid bodies. He offers the following as evidence for this claim:

> Of the experimental reasons which warrant this assumption I will mention only one. The phenomenon of the propagation of light in empty space assigns a tract [distance between

[214] See §1.4.1.1.
[215] See (Rynasiewicz, 2005) for an account of this correspondence.
[216] This point is noted by Ryckman (2005, pp.61-2).
[217] This is strongly suggested by the way in which he introduces the objection that rigid bodies are not suitable candidates to be considered fundamental epistemological entities: he asks why "Poincaré *and other investigators*" (p.236, my emphasis) object to this understanding of ideal rods.

> two points on a rigid rod], namely, the appropriate path of light, to each interval of local time, and conversely. Thence it follows that the above assumption for tracts must also hold good for intervals of clock-time in the theory of relativity. Consequently it may be formulated as follows: if two ideal clocks are going at the same rate at any time and at any place (being then in immediate proximity to each other), they will always go at the same rate, no matter where and when they are again compared with each other at one place. If this law were not valid for natural clocks, the proper frequencies for the separate atoms of the same chemical element would not be in such exact agreement as experience demonstrates. The existence of sharp spectral lines is a convincing experimental proof of the abovementioned principle of practical geometry. (Einstein, 1954, pp.237-8)

This is a version of the past-history objection that Einstein initially made to Weyl in 1918. The objection runs as follows. Consider two, e.g., hydrogen atoms that are brought together so as to compare spectral emissions. To begin with, both atoms emit sharp spectral lines at the same frequencies. They are then sent away, via different paths and brought together at the same point again. On Weyl's theory, Einstein thought, we would expect to see the spectral lines at different frequencies because the electromagnetic field would be different along each of the paths of the two atoms. The fact that this is not observed, Einstein claims, can only be explained by the fact that any two "tracts" and clocks that are the same length or read time at the same rate in one place they continue to do so wherever they are placed.[218]

Here, then, it is clear that Einstein considered the position outlined in his lecture as an alternative to Weyl's account of the role of rigid rods in general relativity. This serves as a strong motivation to read 'Geometry and Experience' in the context of the Reichenbach-Weyl dispute about the status of the metric in general relativity. In light of this, I do not think that the lecture serves as strong evidence for Friedman's claim that the philosophy of Helmholtz and Poincaré played a crucial role in the rotating disk thought experiment: it is more likely, I think, that Einstein referred to the dispute between Helmholtz and Poincaré only as an earlier—and simpler—incarnation of the dispute that he was engaged in. While Friedman is right that Helmholtz's view of geometry does play a role in the rotating disk thought experiment, there is no need to treat Einstein as motivated by this because, as we have seen, the kinematical account of rigid bodies was being considered in the contemporary physical literature with which Einstein was well-acquainted.

[218] Ryckman (2005, pp.87-8) details Weyl's response to this objection: in short, Weyl objected that Einstein needed to explain how physical bodies whose behaviour was indicative of the gravitational field could at the same time be used as instruments to stipulate metrical relations. From the present perspective the outcome of this dispute is not hugely important: what is important is that Einstein's repetition the pre-history objection against Weyl in 'Geometry and Experience' as evidence for his view of the role of rigid bodies, strongly suggests that Weyl was the primary target of the lecture.

There is a further reason for caution before endorsing Friedman's account of physical geometry: in his answer to CC, Friedman is clear that he seeks to move away from treating constitutive principles as coordinating principles that connect uninterpreted mathematical frameworks to physical experience. In taking the rotating disk thought experiment to rely upon Schlick and Reichenbach's understanding of physical geometry, Friedman's account of the development of general relativity still seems to be based upon the idea that we must coordinate mathematical frameworks with experience. Friedman is explicit in his claim that the rotating disk thought experiment plays a crucial role in grounding the constitutive role of the equivalence principle (2010b, p.186); it is difficult, therefore, to see how Friedman's account of the rotating disk thought experiment is consistent with his expressed desire to remove coordinating principles from his account.

We now have two good reasons for doubting that the philosophy of Helmholtz and Poincaré played a central role in Einstein's thinking in 1912. First, it is not necessary to appeal to their philosophies in order to explain the role of the rotating disk. It is adequately explained by (i) a desire to treat rotation as rest so as to increase the number of admissible coordinate transformations and (ii) the investigations of Herglotz into the nature of rigid bodies. Second, the emphasis on Helmholtz and Poincaré in 'Geometry and Experience' is best read, at least in part, as a response to Weyl's pure infinitesimal geometry. If we are to develop Kantian answers to CR and CC, they must both, then, be of a different form to those offered by Friedman.

4.3.2.2. The rotating disk and the rationality of science

In this section I begin by clarifying that our rejection of the claim that there is a role for philosophy in the rotating disk means that we must also reject Friedman's response to CC. I then turn my attention towards developing an alternative Kantian account of the role of the rotating disk in the development of general relativity. I argue that both the rotating disk and the equivalence principle (EP1 and EP2) should be understood as playing primarily regulative roles in the development of the final version of general relativity.[219]

What, then, of Friedman's claim that the equivalence principle is constitutive of general relativity in virtue of the principle's role in the rotating disk thought experiment? Friedman offers a historical narrative describing how the mathematical structure of general

[219] Though there remains a sense in which they are constitutive of general relativity in the sense that they played a historical role in making the laws of the new theory possible. This underlines an important feature of the regulative account of constitutivity: whether a principle is considered to be constitutive becomes very much a question of historical context and of carefully identifying the precise role that a principle played in particular stages of the development of a theory. EP1 and EP2, I suggest, are constitutive of general relativity in a different sense than EP3 is.

relativity gains its empirical meaning (2010a, pp.691-3). How does this account sit with the analysis of §4.3.1? Friedman's argument hinges upon the claim that the rotating disk thought experiment made a variably curved four-dimensional geometry a genuine physical possibility. Given the importance that Friedman places on Einstein's account of the rotating disk in 'Geometry and Experience', it is reasonable to assume that he broadly ascribes to the distinction between pure and physical geometry that Einstein draws in this work. For Friedman, then, I take it that the rotating disk thought experiment makes four-dimensional space-time a genuine physical possibility in the sense that it motivated the abandonment of the three-dimensional approach and the adoption of the line element—determined by measurements with rods and clocks—as the physically meaningful quantity.

From a regulative perspective, of course, the problem with Friedman's approach is that Friedman assumes a relationship between mathematical and physical possibility that has not fully taken to heart the importance of the group-theoretic approach to mathematics. That is, frame-invariant quantities do not need to have their physical significance explained in terms of measurements with rigid bodies and ideal clocks. As we saw in our discussion of Weyl's approach to general relativity in the previous section, the line-element is physically meaningful *in virtue* of its frame-invariance and, furthermore, *determines* the behaviour of rods and clocks. In what remains of this section, I construct an alternative account of how Einstein understood the relationship between mathematical and physical possibility in the early stages of the development of general relativity. I will argue that both the equivalence principle (EP2) and the rotating disk played important roles in making the mathematical possibility of four-dimensional space-time a physical possibility; however, they did so in a manner that means they should not be considered as constitutive principles.

In §4.3.1 I emphasised that Einstein's chief conceptual difficulty in developing general relativity was as to how he should interpret coordinate systems. Why did Einstein have such difficulty in freeing himself from the idea that coordinates have a direct physical significance? Norton (1989b) provides a plausible answer to this problem: whereas contemporary physicists understand differentiable manifolds as a bare topological space, Einstein understood the manifold to be imbued with a much richer intrinsic structure.[220] How did this impact on Einstein's understanding of how physically possible space-times should be understood?

[220] The distinction between Einstein's understanding of manifolds and the contemporary understanding is detailed in (Norton, 1989b).

First, let us clarify the contemporary canonical form of space-time theories,[221] as detailed in Norton's (1989b).[222] Space-time theories have three types of structure associated with them, which Norton identifies as follows:

> *M1.* *Physically possible space-times,* one of which will be the physically actual space-time of our world if the theory in question is true.
>
> *M2.* *Geometric structures,* which are mathematical objects such as $\langle M, g_{ab}\rangle$ or $\langle M, g_{ab}, T_{ab}\rangle$. They represent the space-times of M1.
>
> *M3.* *Coordinate representations,* which are mathematical objects such as $\langle A, g_{ik}\rangle$ (or $\langle A, g_{ik}, T_{ik}\rangle$)). They are the component representations of the structures of M2 in some coordinate chart. Here A is an open set of R^4, g_{ik} a 4 x 4 matrix of components, and the relevant coordinate chart is a diffeomorphism x^i from some neighborhood of the point set of *M* onto *A*. (Norton, 1989b, p.1223)

This is an entirely general description of space-time theories: Newtonian space-time, and the space-times of special and general relativity can each be formulated in this manner.[223]

My interest in this lies in how the relationship between M1, M2 and M3 differs on the modern view from on Einstein's view. On the canonical view, then, M2 represents M1 and M3 represents M2: however the type of representation is quite different. The representation of M1 by M2 is physical: the physically possible space-times of M1 are represented by the differentiable manifolds of M2. However, the representation of M2 by M3 is a matter of mathematical definition: $\langle A, g_{ik}\rangle$ is a subset of $\langle M, g_{ab}\rangle$. So, coordinate charts on the canonical view are true as a matter of definition. This distinction is important because it enables us to distinguish two types of coordinate transformation: active and passive. Passive coordinate transformations represent a redefining of the representation relationship between M3 and M2: they have no physical content. Active diffeomorphisms, however, drag-along the tensor field, meaning that a different metrical field is defined at

[221] All theories of space-time treat it as a four-dimensional differentiable manifold on which geometric objects can be defined: i.e., they are of the form $\langle M, O_1,\ldots,O_n\rangle$. The models of a particular space-time theory are then those that satisfy that theory's laws *L*. On the canonical view, a differentiable manifold is a topological space of points and a set of smooth maps which map the manifold's open sets onto the open sets of R^n: these maps are known as coordinate charts because they coordinate the point set with the set of n-tuples of real numbers. See (Friedman, 1983).

[222] In this article Norton argues that if we read Einstein by imposing the contemporary canonical theory of space-time onto him, it becomes very difficult to understand, e.g., why Einstein seems to have confused active and passive coordinate transformations and why he took general covariance to lead to a generalised principle of relativity. However, if we take Einstein to be operating with a quite different understanding of space-time theories we can explain these problems without resorting to treating them as simple mistakes.

[223] See (Friedman, 1983).

the same manifold point in the two models:[224] active diffeomorphisms correspond to the relationship between M2 and M1, so have physical content.

It is natural to read Einstein's talk of coordinate systems as being analogous to the contemporary idea of coordinate charts. However to read Einstein in this way is to read him as being confused on some key points: in particular, it is not clear why he would take general covariance to lead to a generalized principle of relativity, nor is it clear whether he understood coordinate transformations to be understood as the equivalent of active or passive transformations. Norton explains that this interpretative challenge is the consequence of the fact that Einstein did not understand the manifold as a differentiable manifold; he understood it as the open set of R^4.[225]

R^4 has a richer intrinsic structure than contemporary differentiable manifolds. The two-step contemporary coordinate representation is necessary because there is a distinction between differentiable manifolds as general mathematical objects and R^n, which is a special case of a manifold. The manifold, as a general mathematical object, has a point set of unspecified elements that must be coordinated with R^n. Now, if Einstein understood the manifold simply to be R^4, then he would have no need of the relationship between M2 and M3 that is intended to relate the point set of M to R^n. For Einstein, then, physically possible space-times were represented by models $\langle$A, $(O_1)_{ik\cdots}, \ldots, (O_n)_{il}\rangle$ where the manifold A is an open set of R^4 and $(O_1)_{ik\cdots}, \ldots, (O_n)_{il}$ are n matrices.[226] Objects of the form $\langle$A, $(O_1)_{ik\cdots}, \ldots, (O_n)_{il}\rangle$, then are direct coordinate representations of the possible space-times. In effect, then, Einstein's understanding of coordinate representations contains both M2 and M3 and this is why, from a contemporary perspective, there is no clear distinction between active and passive transformations in his work.

Now, this understanding of manifolds would have left Einstein with an interpretational challenge: the structural features of R^4 admit of a quite natural physical interpretation. So, simply by representing space-times with models of the form $\langle$A, $(O_1)_{ik\cdots}, \ldots, (O_n)_{il}\rangle$, an implicit limitation is placed on the class of physical possible space-times. The most noteworthy examples of this are as follows:

(a) R^4 has an origin (0, 0, 0, 0). This is naturally interpreted as space-time having a preferred central point.

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224 For a tensor field O defined on a manifold M and a diffeomorphisms d, we can define a new tensor field d^*O on M. The

225 This, as Norton points out (p. 1236), was precisely how Riemann, Klein and Levi-Civita understood the manifold in their early on differential geometry. These were the sources that Grossman directed Einstein to in order that he may

226 See Norton (1989b, pp1236-7)

(b) R^4 is inhomogeneous, i.e., each point is distinguishable. This is naturally interpreted as implying that space-time points are distinguishable.

(c) R^4 permits a definition of absolute simultaneity: x_1, x_2, x_3 represent spatial coordinates and x_4 represents instantaneous snapshots of this three dimensional space.

(d) R^4 has a preferred rest frame: the curves of constant x_1, x_2 and x_3 pick out a state of rest.

(e) In R^4 coordinate differences have a direct metrical significance: they correspond to measurements with ideal rods and clocks.

Einstein began to study Minkowski's four-dimensional formulation of special relativity around 1910 and by early 1912 he would have been quite comfortable with it.[227] Minkowski's four-dimensional formulation of space-time was seen as validation of Klein's *Erlangen* program: he showed that special relativity described the space-time geometry of the Lorentz group. This suggests that at this time, Einstein would have been quite familiar with the methods of differential geometry necessary to view space-time in the manner just detailed. Importantly, he would also have been familiar with Klein's fundamental insight: i.e. that each geometry has associated with it a class of admissible coordinate systems with a group of transformations between them, the invariants of the group of transformations represent physically real quantities. This is crucial, because it is this that will allow Einstein to strip away features of the implicit physical interpretation of as $\langle A, g_{ik}\rangle$ and replace it with a new physical interpretation.

By 1912, then, Einstein, by considering invariants of special relativity, would have removed (a), (c) and (d) from his physical interpretation of the manifold plus metric. (a) is eliminated by the Galilean group: classical physics permits a number of coordinate transformations and (0, 0, 0, 0) is not an invariant of these transformations. (d) is also eliminated by the Galilean group: what is defined as a rest state is not invariant across Galilean transformations. (c) is eliminated in special relativity by the Lorentz group where simultaneity is no longer an invariant of the transformations. This, however, still left Einstein with the objectionable notion of an inertial frame, which remains invariant under Lorentz transformations.

[227] Einstein initially did not see the importance of Minkowski's reformulation of special relativity, reportedly referring to it as a "superfluous display of learning" (see Pais, 1982). In a review talk on special relativity given in Zurich in January of 1911 (see CPAE 3, doc.17), Einstein displays a quite different attitude, stating that it was a "very perspicuous representation of the theory, which essentially simplifies its application". This suggests that by 1910 at the latest Einstein had started to familiarise himself with the four-dimensional mathematical formalism. In the process of responding to Abraham's criticism of the theory of static gravitational fields, Einstein became much more familiar with these techniques.

From this perspective, then, the development of relativity is marked by the process of stripping away implicit physical interpretations from mathematical structures. Post's (1971) account of the development of new scientific theories also places importance upon the the process by which extraneous structure is removed from a precursor theory in the development of a new theory (p.229).Post identifies a series of heuristic criteria which he argues have played a vital role in guiding the development of novel scientific theories. The most important of these is the general correspondence principle: i..e, the requirement that any new theory L should account for the success of its predecessor S by replicating the predictions of S under those conditions where S was well-confirmed. Post argues that to do so, those aspects of S-theory that are not independently confirmed must be removed from the theory. The significance of the rotating disk thought experiment, and the early versions of the equivalence principle can profitably be understood in these terms: i.e., they were used to enable Einstein to broaden the range of applicability of his theory and identify those aspects of the precursor theory that cannot be considered well-confirmed by identifying situations in which they do not hold. The process of stripping-away layers of implicit physical interpretation from mathematical structure, then, is important because it would help to ensure that the new theory corresponds only to the well-confirmed parts of the precursor theory.

The regulative demand to consider as large a group of admissible coordinate transformations as possible, then, plays a crucial role in the process: it serves as a demand to interpret a wider range of space-times as physical possibilities. EP1's role in this process was to enable Einstein to expand the group of admissible coordinate transformations so that inertial frames of reference were no longer invariants of his theory's admissible transformations. The rotating disk expanded the admissible transformations further still, so that measurements with rods and clocks no longer have direct metrical significance: in the move to four-dimensional space-time, coordinate differences cease to correspond directly to measurements with rods and clocks, eliminating (e). This leaves only (b), to still impact on Einstein's thinking.[228]

We have now arrived at a clear sense in which the rotating disk thought experiment served to make four-dimensional variably curved space-time a physical possibility. As we saw in §4.3.1, the rotating disk cannot be understood in three-dimensional terms because, in contemporary terms, a rotating relative space does not have a well-defined frame time. This problem led Einstein to introduce the Gaussian line element as a measure of space-time curvature. Because the line element is invariant under arbitrary transformations, the

[228] As Norton (1989b) shows, (b) played a crucial role in Einstein's difficulty in initially understanding general covariance: in particular, it had a central role to play in Einstein's account of the hole argument.

four-dimensional formalism gave Einstein use of arbitrary coordinate systems, meaning that coordinate differences could no longer be considered as corresponding to measurements with rods and clocks. The rotating frame is important, then, because it allowed Einstein to overcome restrictions that his understanding of the manifold placed on physically possible space-times: i.e., that they be of spatial dimensions and one temporal dimension and that coordinate differences correspond to measurements with rods and clocks.

How does this impact on Friedman's account of constitutivity? Friedman's account of constitutivity is based on the idea that the mathematical formalism used to express general relativity lacks a natural physical interpretation: constitutive principles are those that have played a historical role in providing a physical interpretation to the increasingly abstract mathematical formalism of space-time theories. If we understand the development of general relativity as I have suggested in this section, then this helps us to clarify precisely why constitutive principles are needed to play this role. The mathematical formalism that Einstein was using in developing general relativity came with a very natural physical interpretation: $\langle A, g_{ik} \rangle$ is naturally interpreted as a space-time with a preferred central point, preferred rest states and inertial frames and in which coordinate differences have a direct metrical significance. In developing general relativity, Einstein came to see that each aspect of this natural physical interpretation of $\langle A, g_{ik} \rangle$ was mistaken. What is interesting about the development of general relativity, from a Kantian perspective, is the process by which elements of the mathematical structure of Riemannian geometry come to represent particular physical processes. My account differs from Friedman's in that I understand this process as more two-way than Friedman does. For Friedman the mathematical structures are abstract and come with no natural physical interpretation. On my account when Einstein was first applying Riemannian geometry to space-time, he understood the mathematical structures to be such that they naturally represented features of space-time as it was understood prior to the theory of relativity. This means that in the early stages of developing general relativity, Einstein's task was to strip away those elements of the mathematical structure that do not correspond to anything that we do not wish to represent. This is where regulative principles play a significant role: they expand the range of physical situations that we require our mathematical structure to represent and then our understanding of the mathematical structure is adjusted accordingly.

So, initially Riemannian geometry had a natural physical interpretation, however as general relativity developed this was gradually removed. As this process was carried out, it required a constant re-working of the conceptual framework of the theory, and it is in this re-working of the conceptual framework that constitutive principles play an important role. So, for instance, consider the effect of EP1: as Einstein initially understood the manifold of Riemannian geometry, it was naturally interpreted as giving preference to inertial frames of reference. EP1 was important, in its regulative function, because it enabled Einstein to see that the mathematical structure needed to be able to represent physical situations in which inertial frames were not preferenced. This, though, left Einstein with a conceptual difficulty in his attempt to develop a relativistic theory of gravitation: i.e., special relativity made a sharp distinction between inertial and non-inertial motion and a relativistic theory of gravitation could not do this. This problem, as we have seen, was ultimately resolved by strengthening EP1 to EP2 and then representing gravitation as part of the space-time structure rather than a field that is defined on to space-time.

Here, I suggest that EP1/EP2 should be understood as playing a dual role: it plays a regulative role in the sense that it broadens the class of acceptable transformations but it also plays a constitutive role in that it suggests that gravitation be represented as a space-time structure. This consideration of EP1 helps us to delineate the constitutive and regulative roles of physical principles. In their regulative role, I suggest, physical principles broaden the range of physical situations which must be represented by the mathematical formalism: this is destructive in the sense that it exposes the inadequacy of the previous understanding of the representation relationship between mathematics and physics. The constitutive role of principles is constructive in that it suggests a novel physical interpretation of geometrical structure. This can be further clarified by consideration of the role of the rotating disk and EP3 in general relativity.

I suggest that the rotating disk thought experiment functions as a regulative demand on the construction of general relativity and that EP3 serves as a constitutive principle that resolves a conceptual problem raised by the rotating disk. The rotating disk, as we have seen, serves a regulative function in that it broadens the class of accepted transformations so as to include uniform rotations. The consequence of this was that coordinate differences in the manifold could no longer be understood as corresponding to measurements with rods and clocks. This posed a serious problem for understanding the physical representation of mathematical coordinate differences: how could mathematical structures be used to represent physical situations when it was not even clear how physical distances should be represented in the mathematics?

The problem is eventually solved by assigning EP3 a constitutive role. However, the precise sense in which EP3 could play this role would not actually become clear until Weyl had developed the notion of a manifold with an affine connection and interpreted affine geodesics as the paths of freely falling test particles.[229] EP3, recall, is the claim that gravitational and inertial effects are identical in nature and an inertio-gravitational field is responsible for both. It is this that enables a non-flat affine structure to be associated with the behaviour of freely-falling bodies. This, then, provides a sense in which EP3 is constitutive of general relativity: a geometrical notion—an affine geodesic—is associated with a particular observable process.

This understanding of the constructive role of constitutive principles helps us to clarify Friedman's claim in the *Dynamics of Reason* that constitutive principles determine the conceptual framework of a theory. On my account a theory is developed by analysis of the concepts of the old theory, combined with a regulative demand to expand the range of applicability of these concepts. In this section I have sought to show that this process leads to difficulty in interpreting the sense in which mathematical structures represent physical processes: the conceptual framework of the old theory becomes inappropriate for understanding the relationship between mathematics and physics. I have primarily been concerned with the constitutive role of the equivalence principle. I have demonstrated that EP3 was ultimately developed through analysing the Newtonian concept of acceleration: in the course of this analysis EP3 came to represent a novel concept—the inertio-gravitational field—which is eventually required to physically represent mathematical structures. In relying on EP3 in this way, general relativity puts the idea of an inertio-gravitational field at its very centre and, in this sense, EP3 determines an aspect of the conceptual framework of general relativity.

Friedman's objection to a contemporary Kantianism that emphasises regulative principles is that one cannot distinguish constitutive and regulative principles without something like Kant's division between sensibility and understanding. This, as we have seen, is difficult: I suggest that the distinction between constitutive and regulative principle be drawn on the basis of the historically contingent nature of the role of the principle in the development of a theory. Thus, I have argued that EP1 and EP2 are, from the perspective of the final theory of general relativity best understood as regulative and constitutive. They are regulative in the sense that they expand the class of admissible

[229] Here again it is worth noting that the precise nature of the constitutive role of a physical principle only became entirely clear after the construction of novel mathematical techniques. This implies that the relationship between mathematical structures and the physical processes that they represent is not quite as mysterious as Friedman takes it to be. Indeed, I would suggest that—when Weyl's affine geometry was available—the "coordination" of free-fall trajectories with affine geodesics was entirely natural when understood as guided by the dynamical character of the metric and the requirement of general covariance. See (Ryckman, 2010, pp.463-4) for detailed discussion. Without reference to affine geometry, there remains a sense in which EP3 played a constitutive role: i.e., it aided the construction of the conceptual framework of general relativity in that it permitted the metric-tensor to be interpreted either as describing inertial or gravitational effects. However, the constitutive role principle can be seen more precisely if we permit ourselves to appeal to Weyl's mathematical methods.

coordinate transformations and constitutive in that they played a historical role in making the laws of gravitation possible. EP3, by contrast, is constitutive and not regulative: it determines the conceptual framework of general relativity but does not seem to be have any particular regulative role. Finally, general covariance, I suggest in §4.4 is regulative but not constitutive. So, while distinctions between regulative and constitutive principles can be drawn within the framework of a regulative Kantianism, Friedman is right that it is not a principled distinction.

4.3.3. What role did the rotating disk play in the development of general relativity?

It should now be clear why I prefer the framework provided by Cassirer's regulative Kantianism to explain the development of general relativity to that offered by Friedman. First, we saw in §4.2 that Friedman takes the equivalence principle to have been elevated to the status of a principle. However, the equivalence principle as it initially appeared in Einstein's work (EP1) is consistent with Newton's Corollary VI. EP1, then, does not need to be understood as the result of a process of elevation: Einstein identified EP1 as the pertinent feature of Newtonian physics to resolve an inconsistency between special relativity and Newton's theory of gravitation. There was, then, no need for Einstein to elevate an empirical fact to the status of a principle, because the principle was already familiar from Newtonian physics. Einstein implemented EP1 (and EP2) by treating them as widening the class of admissible coordinate transformations. There is no need to appeal to a philosophical meta-paradigm to explain the adoption of the final version of the equivalence principle (EP3): it emerged via a series of natural extensions of EP1 as general relativity developed. This, then, is quite naturally captured on a regulative reading: the origin of EP1 in Newtonian physics ensures that Newton's theory of gravitation and Einstein's are theories concerned with the study of the same object. The role of the equivalence principle in broadening the admissible coordinate transformations is quite naturally understood within a regulative framework: i.e., more admissible coordinate transformations means a better approximation of objective reality. Similarly, in the case of the rotating disk I have argued that there is no need to assign Einstein any particular philosophical motivation in order to understand the role of the thought experiment in the development of relativity.

In §4.3.2, I develop an account of what it might mean to make a mathematical possibility a genuine physical possibility: I argue that this process should be characterised as one of overcoming limitations that were placed on Einstein's physical interpretation of his theory by his understanding of manifolds. On this account the equivalence principle and the rotating disk both play a regulative role, in that they broaden the group of admissible coordinate transformations. The manifold, on this view, comes with a natural physical interpretation given by the invariants of the admissible coordinate transformations and it is these invariants that gave rise to Einstein's understanding of a coordinate system. As the class of admissible coordinate transformations increased, the number invariants of the transformations decreased permitting alternative physical interpretations of the mathematical formalism. Importantly, there is no need to assign *anything* a constitutive role in this process: the manifold plus metric naturally represents possible space-times: regulative principles are important just for permitting the formalism to serve to represent a wider variety of space-times.

This is the source of my most fundamental objection to Friedman's approach: i.e. that there is no special problem about how to apply group-theoretic mathematics to experience. Indeed, I have argued that Einstein only did not see four-dimensional space-time as a physical possibility because he was naturally physically interpreting features of his understanding of the manifold in such a way that prevented a four-dimensional understanding of space-time. The key to seeing four-dimensional space-time as a physical possibility lay in Einstein's revising his own understanding of the mathematical formalism: as he did so, the class of physically admissible space-times widened quite naturally and unproblematically.

In light of (a) it being unnecessary to assign a role to philosophy in answering CR and (b) the role of the equivalence principle and rotating disk being interpretable as regulative demands, I suggest that Friedman's answers to CR and CC cannot be fully satisfactory. We are now in a position to state an alternative, regulative, answer to CR. I will address CC in the next section.

The first part of a regulative answer to CR must establish that classical physics and general relativity deal with the same subject. I suggest that the process by which general relativity emerged out of Einstein's dialectical engagement with aspects of classical physics can be used to provide an answer to this part of CR. I would suggest—following DiSalle (2006)[230]—that the development of special relativity can be profitably understood as arising from Einstein's dialectical engagement with the classical concept of simultaneity. My account has focussed on detailing how Einstein's investigations into a relativistic theory of gravitation began by incorporating Newton's use of Corollary VI and Galileo's equivalence principle into relativity as EP1. This served as a means to bring to light a conceptual tension between Newton's theory of gravitation and relativity theory, i.e., if the admissible

[230] See footnote 181.

relativistic transformations are expanded to include EP1 then the speed of light is no longer constant. The successive development of the theory was aimed at resolving this tension.

It is not necessarily the case that if a theory, B, has been developed by a process of analysing and transforming the concepts of an older theory, A, then A and B must deal with the same subject. However, the details of the development of general relativity are such that I think we can confidently state that, at least in this instance, A and B *do* deal with the same subject. This is for two main reasons. First, while the concepts of classical physics that feature in Einstein's dialectical engagement are transformed, they are not transformed beyond recognition. For instance, the concept of simultaneity is not abandoned, instead Einstein recognised that our judgment of the relationship between two space-time events is dependent upon our implicit use of a physical phenomenon, i.e., light-signalling. Similarly—while I have argued that EP1 can be found in classical physics and that general relativity is developed by a step-by-step transformation of the principle—EP1 and EP3 are not radically different claims: both secure the universality of free-fall and both identify—to an extent—inertial and gravitational effects. The distinction between the two principles lies in their scope: EP1 is limited to a particular class of inertial and gravitational effects while EP3 is not. Second, classical physics and general relativity both seek to provide an account of the same empirical observations. That is, both seek to provide an account of, e.g., the laws that govern the motions of the planets around the Sun. General relativity, of course, seeks to explain a greater range of phenomena than classical physics—e.g., it includes an explanation of the advance of Mercury's perihelion—but, crucially, it provides an explanation of all of the empirical data that classical physics explained. So, I suggest, that we can say that a later theory, B, and an earlier theory, A, deal with the same subject if B emerged from a process of dialectical engagement with problematic concepts in theory A and (i) the concepts in question are not transformed beyond recognition and (ii) theory B provides a lawful explanation of all the empirical phenomena explained by theory A.[231]

We are now in a position to answer CR in the following fashion:

CR: (i) Conceptual analysis played an important role in the development of the theory of relativity. Furthermore, there is continuity between the concepts of classical physics and general relativity and general relativity provides an explanation of the same empirical data that classical physics explained. This provides a means to secure the regulative intuition that

[231] The latter condition can be understood as a version of Post's general correspondence principles.

successor and precursor theories are concerned with the same subject matter: the new theory grows from critical engagement with the earlier theory.

(ii) The equivalence principle and the rotating disk thought experiment are both to be understood as regulative demands to broaden the group of admissible coordinate transformations. It is rational to accept a theory that endorses additional coordinate transformations, as this represents a better approximation of objectivity. Furthermore, the role of conceptual analysis helps here, because it provides a means to clarify conceptual tensions in the old framework and show how the new framework resolves them.

4.4. General covariance as a regulative principle

In this section I emphasise the importance of general covariance for a regulative Kantianism: general covariance is a regulative principle that serves to define a notion of <objectivity-at-a-time> and, thus, is *the* fundamental explanatory concept in general relativity. In this section I defend this understanding of general covariance against well-known complaint that general covariance cannot serve as a fundamental principle in general relativity because it lacks physical content. I also address Ryckman's understanding of general covariance as a regulative *and* constitutive principle and clarify that, on my reading of Cassirer's regulative Kantianism, it would be a mistake to regard general covariance as a constitutive principle.

In §4.2, I argued that in the early stages of the development of general relativity, the equivalence principle should be understood as a regulative principle: i.e., it enabled Einstein to treat an extended class of frames of reference as physically equivalent. However, in the later stages of the development of the theory—after Einstein had adopted EP3, probably by around mid-1912—this role of the equivalence principle was taken over by a generalized principle of relativity, which Einstein sought to implement by making his theory generally covariant. Einstein now understood the equivalence principle as EP3: the claim that inertia and gravitation are of essentially the same nature and that the metric g_{ik} refers to the inertio-gravitational field.[232]

[232] That Einstein understood the generalized principle of relativity and the equivalence principle as distinct is most clear in his (1918), where—with Mach's principle—they are listed as two of the foundational principles of general relativity. That he understood them as distinct by mid-1912 is suggested by the fact—as we will see—that the two principles played quite different roles in Einstein's search for gravitational field equations in the Zurich Notebook.

Let us begin by examining the role of general covariance in the development of general relativity. Einstein general covariance to be of great significance: it played a crucial role in his investigations in the Zurich notebook and remains central to his (1916) exposition of general relativity. In the latter work, Einstein makes two demands of the laws of physics:

(1) Generalised Principle of Relativity: "*The laws of physics must be of such a nature that they apply to systems of reference in any kind of motion*" (1916, CPAE 6, p.149).
(2) Principle of General Covariance: "*The general laws of nature are to be expressed through equations which hold good for all systems of co-ordinates, that is, are covariant with respect to any substitutions whatever (generally co-variant)*" (*ibid.*, p.153)

Einstein went on to state that it was clear that a theory which satisfies (2) will also satisfy (1). Einstein's connection between a generalised principle of relativity and general covariance has long been a cause for dispute.[233] It is now understood that it is a mistake to treat general covariance as securing the general relativity of motion.[234] I do not hope to resolve all the questions connected to the significance of general covariance within general relativity: I mean to show only that general covariance can be profitably understood as playing a regulative role in the development of relativity theory between 1912 and 1916. Ryckman (2005, ch.2) offers a similar account of the significance of general covariance; however, as we have seen in §3.3.2, he argues that general covariance is at once a regulative *and* a constitutive principle.[235] In this section I first seek to show, *contra* Ryckman, that general covariance should not be understood as playing a constitutive role in general relativity: in doing so, I aim to shed light on the significance of the equivalence principle in this period.

I have characterised the development of relativity as being primarily driven by Einstein's removing layers of structure from space-time that had been implicitly imposed upon it by his understanding of the manifold. Of particular importance was Einstein's struggle to remove the privileged status of inertial frames of reference. EP1, EP2 and the rotating disk played an important role in the development of relativity, but none of these

[233] See (Norton, 1993) for the standard account of the "eight decades of dispute" that general covariance sparked.
[234] See (Friedman, 1983) for the standard account of why this is so: the problem is that demanding that the laws of nature take the same form in every frame of reference is much too weak a criterion to express a relativity principle. The connection between covariance principles and relativity principles does hold in the context of flat space-times with privileged inertial frames of reference, however, as soon as we move to the dynamical space-time of general relativity the connection breaks.
[235] See, especially, footnote 156.

fully undermined the privileged status of inertial frames: the equivalence principle just extended the privileged status to uniformly accelerating frames of reference, and the rotating disk extended it to uniformly rotating frames of reference. To be sure, both the equivalence principle and the rotating disk played an important role in the process, but it was only with general covariance that Einstein completed it.

While general covariance was important for this reason, Einstein's (1916) understanding of it included two significant confusions. The first confusion is the claim that general covariance entails the generalised principle of relativity. As we have seen Einstein initially introduced EP1 as a means to extend the relativity principle and he implemented it by treating EP1 as expanding the class of admissible coordinate transformations. In the early stages of general relativity this is possible because Einstein was working within the context of three-dimensional Euclidean space. General covariance, like EP1, expands the class of admissible coordinate transformations: it would have been perfectly natural to assume that, just as EP1 served as a relativity principle, general covariance would too. However, with the move to four-dimensional variable space-time it was no longer permissible to treat general covariance as a relativity principle.[236]

The second problem relates just to Einstein's statement of the principle of general covariance. Here, Einstein treated general covariance as the claim that the laws of nature take the same form in every frame of reference. In 1917 Erich Kretschmann argued that this did not amount to more than a challenge to the ingenuity of mathematicians.[237] The problem is that general covariance, understood as the demand that the equations that express the laws of nature take the same form in every frame of reference, amounts to the claim that they preserve their form under arbitrary transformations between coordinate charts. This is known as a "passive" transformation: it does not change the physical system in question but merely re-describes the physical system in a different coordinate chart. Treating general covariance as the demand that equations maintain the same form for all

[236] Einstein seems to have thought that the equivalence principle (EP3) would serve to enable him to treat general covariance as a relativity principle by providing a means to physically interpret different states of motion in different frames of reference. However, while EP3 captures an important intuition—i.e. that the effects of a gravitational field cannot be distinguished from inertial effects—it is not strictly true: in modern formulations, a gravitational field is present so long as the Riemann tensor is non-vanishing: this is something that *all* observers should agree on. So, when we move to the four-dimensional treatment of relativity, the equivalence principle can no longer be considered rigorously valid. As such, if this was meant to play a role in Einstein's thinking that general covariance entailed a relativity principle, it is no wonder that the entailment fails in four-dimensional variably curved space-times.

[237] As Norton (1993; 2003) points out, while this is often how Kretschmann's objection is parsed, Kretschmann actually intended to make different point. Einstein attributed physical significance to space-time coincidences; Kretschmann showed that if one takes the catalogue of space-time coincidences to exhaust the physical content of general relativity, then this is not a feature that is unique to general relativity and it is for this reason that any space-time theory could be given a generally covariant formulation.

coordinate systems, then, does not tell us anything of the behaviour of physical systems: it is a purely mathematical demand.

Unlike coordinate transformations, manifold transformations are "active":[238] active transformations do not effect coordinate labels but instead act on points so that the geometrical objects defined on the manifold change. The active transformations that we are concerned with are diffeomorphisms. Diffeomorphisms are maps between two manifolds, so if we have manifolds M and N then a diffeomorphism is a one-to-one C^{∞} mapping[239] d from an open subset $A \subseteq M$ onto an open subset $dA \subseteq N$.[240] Diffeomorphisms, then, are more physically interesting than passive transformations because they can spread the metric in a variety of different ways within the same coordinate system. The problem with Einstein's (1916) statement of general covariance should now be clear: he seems to take general covariance to be concerned with passive transformations, whereas general covariance is better understood in terms of active transformations.[241]

In §3.3.2, I suggest that a regulative Kantianism is best understood as treating general covariance as defining that which is objective within general relativity. We are now in a position to see how this will work. Corresponding to the distinction between active and passive transformations is a distinction between a theory's covariance and invariance group. The covariance group of a theory is that which specifies all admissible coordinate transformations, while the invariance group of a theory is that which remains constant under all active transformations: these invariants are standardly referred to as the absolute objects of the theory.[242] The absolute objects of a theory are meant to be those that are unaffected by any interaction with the other objects of the theory. These objects are to be contrasted with the dynamical objects of a theory, which *are* affected by their interactions with other objects. For instance, the metric of special relativity is an absolute object because it is Lorentz invariant and special relativistic object do not alter it. In general relativity the invariance group is the group $Diff(M^4)$ of all diffeomorphisms of the space-

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238 See (Friedman, 1983, pp.46-61).

239 A mapping m: $A \rightarrow R$ (where A is an open subset of R^n as introduced in §4.3.2.2) is C^{∞} iff m possesses continuous partial derivatives of all orders on A. See (Friedman, 1983, p.340)

240 See (Friedman, 1983, p.358)

241 This is not to say that Einstein was not aware of active transformations. As Norton (1989b) stresses, Einstein used the term "coordinate transformation" ambiguously: in some cases it is better to read him as meaning passive transformations and, in others, e.g. the hole argument, as meaning active transformations. The chief advantage of taking Einstein to have understood the manifold as R^n, as suggested in §4.3.2.2, is that it explains this ambiguity. In the modern formulation there are two clearly distinguished representation relationships: i.e. between coordinate representations and the geometric structures and between the geometric structures and physically possible space-times. These correspond to passive and active transformations respectively. In treating the manifold as R^n, Einstein effectively ran together these two aspects of representation and, in doing so did not have the means to so clearly distinguish between active and passive transformations.

242 The terminology originates with Anderson (1967), Friedman (1983) offers a more accessible treatment of the subject.

time manifold: the metric of general relativity is a dynamical object as it is not invariant under this transformation and is determined by matter-energy sources.

Diffeomorphism invariance can be usefully understood as *background independence*:[243] from the present perspective this is helpful because it clarifies the regulative role of general covariance. General relativity is background independent in the sense that the metric is not determined with respect to any background structure: it is determined wholly by the mass-energy distribution and evolves dynamically.[244]

On a regulative Kantianism, this methodological requirement to remove the background-dependence of our theories is well placed to provide an account of the continuing role of background independence in physics. In the previous chapter I suggested that the regulative ideal towards which science aims should be understood as a maximally unified theory that embodies the concept of objectivity-in-general. Understanding general covariance as background independence clarifies the manner in which general covariance can serve as an approximation of this idea of objectivity that finds expression in general relativity.. The demand for background independence serves to ensure that there is no preferred frame of reference. The background-independent laws hold in every frame of reference and, in this sense, are objective.

Ryckman argues that general covariance serves as "an *a priori* constitutive, yet guiding regulative requirement to be placed on the conception of physical objectivity" (2005, p.24). On the account above, the sense in which it is a regulative demand is clear: it ensures that the validity of the laws of nature is to be determined independently of any given preferred frame of reference. However, it is not clear that this amounts to the principle being *constitutive* of physical objectivity. From the perspective of the regulative Kantianism developed in §3, there is some ambiguity in Ryckman's statement about whether general covariance should be understood as constitutive of the concept <objectivity> or of the concept <object> in general relativity. It does not make sense to speak of general covariance as constitutive of <objectivity>: the concept <objectivity> is not made possible by general covariance, it just approximates the regulative ideal of

[243] See (Giulini, 2006) for a detailed examination of the relationship between background independence and general covariance. That general relativity is background independent is a consequence of Anderson's argument that general relativity has no absolute objects: any fixed and permanent background for general relativity would have to be invariant under the group *Diff*(M^4).

[244] Background independence continues to play a guiding role in attempts to develop a theory of quantum gravity. For instance Smolin (2005, p.204) argues that physics advances "*by identifying the background structure in our theories and removing it, replacing it with relations which evolve subject to dynamical law*". There has been some dispute over whether a theory that seeks to unify quantum theory and general relativity should be background independent or not. Some approaches—i.e. string theory and peturbative quantum general relativity—are background dependent. However many of the more promising approaches—e.g., loop quantum gravity and background-independent string theory among others—prioritise background independence. Smolin, at least, urges that the background-independent approaches are to be preferred.

<objectivity-in-general>. Can general covariance be understood as being constitutive of objects of experience? This does not really make sense either. The natural way to understand this claim would be to say that general covariance is constitutive of those objects that are diffeomorphism invariant: however, as we have seen, general covariance is epistemologically significant for general relativity insofar as it amounts to background independence, which is just the denial that there are any diffeomorphically invariant objects. Within my regulative framework, then, general covariance cannot be constitutive of objects of experience.[245]

In this section I have sought to show, against the sort of concerns expressed by Friedman (1983) about taking general covariance to be a principle associated with general relativity, that there is a sense in which general covariance can be understood as a regulative demand that is instantiated by general relativity: understood as background independence there is a clear sense in which general covariance is a regulative principle and in which general relativity instantiates the principle in a way that earlier space-time theories does not.

4.5. A regulative reading of the development of general relativity

In this chapter I have primarily been concerned to defend a regulative answer to CR. My account is intended to respond to Friedman's two main objections to a regulative Kantianism: (1) that a regulative account cannot explain the forward-looking rationality of theory change and (2) that a regulative Kantianism, in abandoning Kant's distinction between sensibility and understanding, cannot answer CC. I have argued that both these challenges can be met and, furthermore, that the regulative reading, with its emphasis on invariance principles, provides a more convincing philosophical account of the development of general relativity than Friedman's constitutive reading.

How, then, should a regulative Kantianism answer CR? As was the case for Cassirer, there are two aspects to this question. First, it is important to explain how it is that we can be sure that the sequence of scientific theories are dealing with the same subject. In the previous chapter we saw that Cassirer sought to answer this question by arguing that successive space-time theories all seek to reveal the ultimate invariants of experience, e.g., the notion of spatiality in general. While this remains defensible in its broad outline—i.e., general relativity refines the Newtonian conception of space—I do not think that this is the best way to address this part of CR. Cassirer understood the ultimate

[245] This is not to say that Ryckman's understanding of general covariance does not make sense within his own account of constitutivity: I claim only that when one reads Cassirer as I have advocated in ch.3, it makes no sense to treat general covariance as constitutive and regulative.

invariants of experience to be ultimately constitutive of every judgment in the sequence of space-time theories: this does not seem plausible in light of the account of general relativity developed here.

I have argued for an alternative. It is possible to secure the regulative intuition that classical physics and general relativity deal with the same subject by examining the process by which the latter theory emerged from the former theory. In §4.3.3, I have argued that when a later theory is developed by a conceptual analysis of an earlier theory—and (i) the concepts in question are not transformed beyond recognition and (ii) theory B provides a lawful explanation of all the empirical phenomena explained by theory A—this is sufficient to conclude that the two theories are concerned with the same subject. This, I suggest, provides a satisfactory answer to the first part of CR.

The second part of CR is as to how we should explain the forward-looking rationality of a theory change: i.e., why would it have been rational for a proponent of Newton's theory of gravity to abandon it in favour of Einstein's? The answer to this lies in the manner in which Einstein developed general relativity. His theory was developed in a step-wise process in which he incrementally increased the class of admissible coordinate transformations. On a regulative reading it is rational to prefer a theory that permits more coordinate transformations because it will offer a better approximation of the regulative ideal. In the case of general relativity, though, there is more to the answer than this: the key steps along the path to general relativity are made by exploring and resolving inconsistencies in the previous framework. DiSalle (2006, pp.103-11) shows that Einstein's definition of simultaneity can be understood as a conceptual analysis of the Newtonian conception of simultaneity.[246] A Newtonian scientist who was aware of the difficulties involved in determining distance-simultaneity could easily see why it would be rational to accept Einstein's definition of simultaneity. In his (1907) paper, then, Einstein displayed another conceptual tension: between his (1905) theory of relativity and Newton's Corollary VI. This was eventually resolved in his theory of static gravitational fields by taking the variable speed of light to represent gravitational potential. However, it was immediately clear to Einstein that his 1912 theory could not be fully satisfactory because it could not provide an account of uniform rotations: this was a problem with the three-dimensional formalism and, as we saw in §4.3, embracing four-dimensional formalism was the most obvious way—given the mathematical tools available to Einstein—to resolve this problem. So, there is a clear path that leads from conceptual tensions in Newton's theory to the

[246] See footnote 181.

general theory of relativity: this, combined with the broader invariance group of Einstein's theory, is surely enough to render the adoption of general relativity prospectively rational.

In the next chapter I consider Friedman's second objection in depth: i.e., that a regulative reading cannot provide a distinction between constitutive and regulative principles. This objection is, as we have already seen hinted at, is going to be more difficult to respond to. I have characterised the equivalence principle as a regulative principle on the grounds that it expands the group of admissible coordinate transformations. In §5 I argue that there is a sense in which the equivalence principle is constitutive of the laws of general relativity. The difficulty in providing an account of constitutivity that is satisfactory from a contemporary perspective is to be expected: after all, constitutive principles were initially understood as synthetic a priori principles that the understanding applied to the bare manifold of intuition provided by the faculty of sensibility. If we wish to develop an account that is meaningfully Kantian, without also accepting the division between sensibility and understanding and the synthetic a priori, then it is inevitable that we must compromise somewhat in answering CC.

5

Objectivity and structuralism in general relativity

5.1. Introduction

In this chapter I argue that Cassirer's answer to CC represents a plausible and attractive contemporary Kantian option. There are two strands to Cassirer's answer to CC: first that the principles of a theory are constitutive of its laws and, second, that the laws of a theory are constitutive of the objects of the theory. With the development of ontic structural realism, there has been increasing interest in Cassirer's law-constitutive account of objects. In particular, Cei and French (2009) argue that Cassirer's account of law-constitutivity can be stripped free of its Kantian roots and used to motivate certain features of ontic structural realism. In this chapter I respond to this claim in two ways: first, I suggest that Cassirer's particular brand of law-constitutivity is tightly interwoven with his Kantianism and, second, that the Kantian account of law-constitutivity is to be preferred to the structural realist account.

I begin this chapter by outlining a potential problem for taking the law-constitutivity of objects to count as a satisfactory answer to CC. In short, the idea that the objects of a theory are made possible by the laws in which they stand is not uniquely Kantian. As Brading has shown, the idea can—in all likelihood—be traced back to Newton and can be used to defend an ontology containing metaphysically real objects.[247] Furthermore, the idea that objects are law-constituted is also central to certain versions of structural realism.[248] Law-constitutivity on its own is too weak to deliver an answer to CC that leaves us with a meaningfully Kantian philosophy of science.

It is this fact that opens the way for Cei and French's attempt to deploy Cassirer's structuralism as an argument for structural realism. They argue that if Cassirer's account of

[247] See (Brading, 2011) for the law-constitutive reading of Newton's account of objects and (Brading and Skiles, 2012) for a contemporary defence of law-constitutivity.
[248] Here I have in mind eliminative ontic structural realism, which I will return to in §5.2.

law-constitutivity is amended so as to utilise a structural account of laws—rather than a neo-Kantian account—then it can be understood in realist terms. In §5.3.1 and §5.4 I identify two features of Cassirer's structuralism that I suggest complicate this move. First, I argue that Cei and French's reading of Cassirer does not do justice to his distinction between the roles of objectivity and physical principles in the structure of scientific theories. This difference is important because on Cei and French's understanding of objectivity it is quite natural to ask whether our invariance principles represent any feature of objective reality. However, on my reading of Cassirer's understanding of objectivity, such questions do not make sense.

Second, I argue that physical principles have a far more significant role to play in Cassirer's philosophy of science than is suggested in Cei and French's reading of Cassirer: in §5.4.1, I argue that principles were of much greater significance to Cassirer than laws. In this section, therefore I clarify the role of physical principles in Cassirer's philosophy of science and argue that, coupled with a law-constitutive account of objects, this amounts to a plausible contemporary Kantian answer to CC.

This, I take it, shows that it is not a straightforward matter to deploy Cassirer's structuralism as an argument for structural realism. This does not, of course, undermine the structural realist account of law-constitutivity: it is simply to say that Cassirer's structuralist framework cannot be straightforwardly stripped away from its neo-Kantian roots and deployed in the service of contemporary forms of structural realism. However, there remains a further problem for the Kantian: why should we defend Cassirer's version of law-constitutivity rather than the structural realist version? In §5.3.2, I suggest that the strongest motivation for resisting structural realism, from a Kantian perspective, is that the conception of the role of philosophy in science that marks the structural realist approach is deeply undesirable from a Kantian perspective. In §5.3.2 I clarify the sense in which philosophy is a branch of science on structural realism and defend, instead, a Kantian conception of the relationship between philosophy and science.

5.2. The law-constitutive account of objects

As we saw in §3.3.2, Cassirer took the laws of a theory to be conceptually prior to the objects of that theory. I argued that, for Cassirer, this means that there is an important sense in which the laws of a theory are constitutive of objects: i.e., for something to be an object of a theory is for that object to conform to the laws of that theory. On Cassirer's account this meant that there are no mind-independent objects: the objects identified by our scientific theories are expected to evolve with our scientific theories. The idea of law-constitutivity, though, is not one that is unique to a Kantian philosophy of science: it has also been deployed in the service of both entity and structural scientific realism. In this section I introduce the law-constitutive account of objects as it features in the contemporary debate.

In contemporary philosophy of science there is increasingly a prevalent attitude that structural realism represents the most promising form of scientific realism. In its most general form, structural realism is the claim that scientific theories are successful not because they refer to actually existing objects but because they refer to structural features of the world. This view was first defended by Worrall (1989), who argues that it enables the realist to do justice to both the pessimistic meta-induction (PMI) and the no miracles argument (NMA). Worrall's structural realism grants, in response to PMI, that the entities posited by past unsuccessful theories—e.g. Fresnel's aether—did fail to refer to any existent metaphysical object. However, we must also be able to account for the success of past theories in spite of this failure of reference: Worrall argues that we may do so taking past scientific theories to correctly identify some aspect of the structural relations between whatever externally existing objects there may be.

On Worrall's *epistemic structural realism* (ESR), both objects and relations are metaphysically real: objects are primary and relations hold between these objects. The characteristic claim of ESR is that while objects do exist, we are only ever able to have knowledge of the relationships in which they stand.[249] French and Ladyman developed *ontic structural realism* (OSR) as an alternative to ESR:[250] they argue that objects do not exist as individuals independently of structural relationships. This attitude towards objects and relations can be understood in either an eliminative or non-eliminative fashion:[251]

Eliminative OSR:	There are only relationships; there no relata that stand in those relationships.
Non-eliminative OSR:	There are both relations and relata that stand in those relations, but relations are primary and the relata are

[249] A proponent of ESR is not committed to the claim that objects exist, it is consistent with ESR to be agnostic about objects: i.e., to claim that there may or may not be objects but that it is impossible to know either way. I am not concerned here with this version of ESR; I argue in this section that a law-constitutive account of objects is the most promising way to understand objects in contemporary philosophy of science and I do not think that the agnostic version of ESR impacts on this argument.

[250] See (Ladyman, 1998) and (French and Ladyman, 2003; 2011) for arguments to the effect that OSR is to be preferred to ESR.

[251] I draw this distinction in the same manner as (Stachel, 2006, p.54).

> secondary. The relata can be understood either as objects or properties.[252]

On eliminative OSR, then, all that there is to know of the world is its structure: objects do not exist as individuals independently of structural relationships: this is the view defended by French and Ladyman.[253] Non-eliminative OSR is reluctant to do away with relata altogether: it is assumed that while relationships are primary there must be something—whether objects or properties—that is also ontologically primitive. The claim that relations are primary to relata should be understood as the claim that at least one essential property of the relata is relational.

What motivates the idea that objects either need to be eliminated from our metaphysics or, at least, be reconceived in relational terms? The main argument supporting OSR is the argument from underdetermination.[254] This argument is based upon the idea that if objects do exist as metaphysical primitives, then there should be a fact of the matter as to whether or not those objects are individuals or non-individuals. Furthermore, if we are to adopt a realist attitude towards these objects, then we would expect our best current scientific theories to accurately reveal whether these objects should be considered as individuals or non-individuals. However, the question of the individuality of objects is metaphysically underdetermined by our best current scientific theories.[255] OSR urges that the appropriate response to this underdetermination is to accept it, and seek the structural commonalities between the two metaphysical options: this is achieved by giving up talk of objects and taking structure to be primary.

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252 See (Lyre, 2011) for a version of non-eliminative OSR according to which properties are ontologically primitive and (Esfeld and Lam, 2008) for a non-eliminative OSR according to which objects are ontologically primitive. See (Ainsworth, 2010) for a taxonomy of approaches to OSR.

253 Brading and Skiles (2012) suggest that there is some ambiguity in their position because they sometimes advocate reconceptualising objects rather than eliminating them, e.g.: "Let us be clear: we are not 'anti-ontology' in the sense of urging a move away from electrons, elementary particles etc. and towards 'observable structures' or the S-matrix or whatever; rather, we urge the reconceptualization of electrons, elementary particles and so forth in structural instead of individualistic terms" (French and Ladyman 2003, p.37). However, here I take French and Ladyman to mean that objects may retain a useful heuristic role in science, but that they need to be understood only in terms of relational properties: i.e., the objects should not be understood as having any intrinsic essential properties. This may seem close to Lyre's (2011) non-eliminative OSR, but there remains a distinction: French and Ladyman are granting that it is reasonable to talk about objects as if they were a metaphysical category so long as they are characterised in solely relational terms, while Lyre thinks that there *is* a metaphysical category of objects. In their most recent article, French and Ladyman are clear that their purpose is to eliminate objects: "One key thesis of OSR is eliminativism about objects…" (2011, p.27).

254 This argument is deployed in (Ladyman, 1998; French and Ladyman, 2003) and has been more recently defended in detail in (French, 2011).

255 The most frequently cited example of a theory underdetermining the individuality of its objects comes from quantum physics, which is can be understood either as treating quantum particles as individuals or non-individuals. See (French, 1989).

Brading and Skiles (2012) argue that a law-constitutive approach to objects can be used to rebut the argument from underdetermination and to defend an account of objects according to which they are conceptually prior to relations: this amounts to the claim that there are objects such that none of their essential properties depend upon the relationships into which they enter. Brading argues that this notion of law constitutivity can be traced back to Newton, [256] who seems to explicitly endorse a law-constitutive understanding of bodies in *De Gravitatione* (Newton, 2004). The law-constitutive approach of Newton, it is argued, provides a viable contemporary option and enables us to reject the argument from underdetermination: it does so by separating questions of objecthood and questions of individuality (p.105). That is, what it is to be an object is no longer tied up with notions of individuality: what it is to be an object, by contrast, is to satisfy a system of physical laws. This means that if, on a particular theory, the laws do not commit us to treating objects as either individuals or non-individuals then it is a mistake to try and provide a metaphysical account of those objects as either individuals or non-individuals. The laws of a theory exhaust all there is to say about the objects of a theory. This means that Brading and Skiles can respond to the argument from underdetermination simply by denying that there must be a fact of the matter about whether or not there objects are individuals.

The law-constitutive approach to objects is very natural from the perspective of the regulative Kantianism developed in §3. From the perspective of developing a Kantian answer to CC, Brading and Skiles's (2012) raises a problem: a Kantian philosophy of science cannot emphasise law-constitutivity alone because this is compatible with an ontology that contains either metaphysically real objects or metaphysically real structures. For law-constitutivity to represent a *Kantian* answer to CC it must be accompanied by an appropriate understanding of laws. In the next section I outline the two key features of a Kantian account of laws that will ensure that a Kantian version of law-constitutivity can be distinguished from both the law-constitutivity of both eliminative OSR and Brading and Skiles: i.e. for the Kantian invariance groups must be interpreted as regulative ideals and there must be a constitutive role for physical principles in making laws possible in the first place.

[256] See (Brading, 2011).

5.3. Structuralist accounts of objectivity and objects

Certain expositions of eliminative OSR take Cassirer's account of law-constitutivity to be amenable to a realist interpretation.[257] In this section I argue that the neo-Kantian understanding of law-constitutivity advocated by Cassirer cannot be straightforwardly deployed by OSR. While both Cassirer and OSR take the notion of objectivity within a particular theory to be secured via invariance principles, I claim that the concept of objectivity actually plays a quite different role in the conceptual structure of OSR and Cassirer's structuralism.

5.3.1. From objectivity to objects

There are striking similarities between Cassirer's structuralism, detailed in §3, and OSR.[258] Both (i) take the concept <objectivity> to be explanatorily prior to the concept <object> (ii) take objects to be constituted by the laws of a theory and (iii) take the laws of a theory to be objective insofar as they encode the relevant invariance group of a theory.[259] The task of this section is to explain the sense in which OSR endorses (i)-(iii) and to clarify how OSR's approach differs from Cassirer's. In a sense this is quite obvious: on OSR the laws of a theory are taken to refer to actual relations that make up the structure of the world, whereas for Cassirer laws are only made possible by the historical role of physical principles in their development and these principles do not refer to any feature of metaphysical reality. However, in light of Cei and French's (2009) argument that the only distinction between Cassirer's structuralism and OSR is their differing account of laws, it is important to be clear about the precise source of this difference between OSR and Cassirer: I argue that it lies in a differing understanding of the relationship between objectivity, principles and laws.

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257 E.g., Cassirer's claim that the laws of a theory are conceptually prior to the idea of electrons is cited approvingly in (French and Ladyman, 2003, p.30; Cei and French, 2009, p.109; Cei and French, forthcoming, p.11).

258 The similarity between OSR and Cassirer's structuralism has been made most explicit in the case of *eliminative* OSR, for the most detailed discussion see (Cei and French, 2009). However, I take it that non-eliminative OSR is similarly committed to the explanatory priority of <objectivity> to <object>: this, I suggest in §5.3.2, is the most natural way to make sense of the claim that while there are both relations and objects relations are primary. It is certainly the case that on non-eliminative OSR, as on eliminative OSR, objectivity is to do with invariance and not to do with objects. For the sake of the main body of the discussion of this section I do not, therefore, distinguish between the eliminative and non-eliminative forms of OSR.

259 Non-eliminative OSR agrees on points (i) and (iii), but it's not clear that all forms of non-eliminative OSR would need to accept that objects are constituted by the laws of a theory. Non-eliminative OSR, though, is at least consistent with a law-constitutive approach to objects. Here, though, I take the law-constitutivity of objects as the starting-point for my analysis and so am only interested in non-eliminative OSR insofar as it takes objects to be constituted by laws.

Cassirer's structuralism plays a quite central role in certain expositions of OSR, which appeal to Cassirer in order to help with key conceptual difficulties. For instance, French and Ladyman (2003, §3) appeal to a modified version of Cassirer's account of objectivity in order to explain how OSR can satisfy the realist's demand for objectivity without objects. Indeed, Cei and French (2009) argue that Cassirer's structuralism—separated from his neo-Kantian account of laws—can be deployed as a means to help clarify certain features of eliminative OSR.

Cei and French take Cassirer's structuralism to be derived from four tenets (pp.112-3):

(1) *Holism*: this is the view that statements of the results of measurements, laws and principles cannot be understood individually, and only make sense in relation to each other.
(2) *Functional Coordination*: As we saw in §3.3, there are two features of functional coordination: first, there is a preference for relational concepts and, second, the starting point of scientific analysis is the concept of <objectivity>. Cei and French do not emphasise this role for the concept of <objectivity> and instead treat the second aspect of functional coordination as determining the manner in which measurements, laws and principles relate to each other. The effect of this is that it is principles—and especially symmetry—that take on the role of securing a sense of objectivity on Cei and French's account.
(3) *The centrality of the notion of law*: Cei and French identify laws as being central to Cassirer's account: it is laws that express the general pattern that is observed in individual cases. They argue that principles play a less significant role because they just replicate the coordinative relationship between laws and singular cases at the more general level of laws themselves.
(4) *Neo-Kantian conception of laws*: Laws are logico-mathematical elements that play a synthetic constructive role in thought in allowing knowledge to be derived from experience.

Cassirer's structuralism, on this view, is not a consequence of his neo-Kantianism. The only explicitly Kantian element in Cassirer's philosophy is his particular understanding of laws. If laws and principles are reconceived in a realist relational fashion, then eliminative OSR can be derived from tenets (1)-(3) of Cassirer's view.

From the perspective of my reading of Cassirer in §3.3, there are two factors that make this process less clean than Cei and French would have it. First, as mentioned, Cei and French run together the distinct roles of the concepts of <objectivity> and <knowledge> in Cassirer's function-theory of coordination. Second, principles have a much more significant role in Cassirer's system than Cei and French allow: I have argued in §3.3.2 that Cassirer takes physical principles to make laws possible and this, I think, means that it is principles that should be understood as central for Cassirer. In this section I focus on how the understanding of <objectivity> differs on the two accounts. In §5.4 I return to the question of the role of principles and argue that, for these two reasons, a realist account of laws and principles cannot be straightforwardly slotted in to replace the Kantian account of laws in Cassirer's system.

Let us turn our attention, then, towards the structural account of laws. One of the central claims of OSR is that objectivity has nothing to do with objects. Instead, OSR seeks to secure the objectivity of scientific laws by taking the laws to be governed by higher level principles: specifically symmetry principles and laws of conservation. The distinction between OSR and Cassirer's structuralism, of course, is that for Cassirer the invariance group of a particular theory is understood as a regulative ideal, whereas for the structural realist symmetry principles—from which invariance groups are derived—capture relevant features of the structure of the world.

It would seem, then, that the structural realist is just as able to reverse the direction of explanation between objectivity and objects as Cassirer was. This should not be surprising: Cassirer argued for function-concepts on the basis of a detailed historical analysis of the development of concepts in physics and while the motivation for this historical analysis was Kantian,[260] the details of it are not. That is, whether the development of physics is viewed from a Kantian or realist perspective it seems equally clear that invariance principles are increasingly taken to be more fundamental than objects. Furthermore, it is also clear from either perspective that in modern physics it is the invariance group of a theory that is taken to be objective.[261]

The idea that objectivity can be understood in terms of invariance—which I will refer to henceforth as *invariantism*[262]—has been criticised by Debs and Redhead (2007), who

[260] That is, Cassirer was seeking to answer the transcendental question as to the possibility of natural science.

[261] We have seen in §3.3, how this works for Cassirer, where <objectivity> is understood as a regulative ideal. This idea, though, can equally well be understood in realist terms. E.g., (Nozick, 2000, pp.75-6) argues that the three strands of our everyday characterisation of an objective fact—accessibility, intersubjectivity and independence—can be quite naturally captured in terms of invariance: i.e., by the claim that "an objective fact is invariant under transformations" (p.76).

[262] Here I adopt Debs and Redhead's terminology (2007, p.60). They characterise invariantism as the commitment to the following three claims. First, is the claim that for something to be objective it must be invariant under symmetry transformations: they trace this idea back to Weyl's claim that "objectivity means invariance with respect to the group of automorphisms" (1982, p.132). Second, they argue that invariantism is committed to the idea that the increased success of theories of broader invariance groups is to be explained by the fact that the theories have a broader invariance group. Finally, they claim that a key tenet of

raise three objections to the position. Here I address the two most pertinent.[263] First, they draw a distinction between accidental and heuristic symmetries and object that there is no obvious way to determine which should be understood as representing the symmetries of nature.[264] Second, they object that invariantism is linked with the idea of unification and that this is a problem because it prevents us having objective knowledge of current theories because true objectivity is embodied in the GUT.

French's (2012, §1.7) response to these objections will prove useful in clarifying precisely how the ontic structural realist understands objectivity. French argues that one should view the GUT as the ideal limit upon which our current theories converge:[265] the idea is that the final unified theory will embody a certain set of universal physical symmetries. Our present theories are successful because they approximately identify some feature of the symmetries of the final theory. So, once we took parity to be a fundamental physical invariance and then quantum physics showed it was violated. However, rather than discarding parity invariance altogether it was absorbed into CPT-invariance. On OSR, parity invariance was useful to past theories because it correctly identified an approximation of a symmetry that was valid within its domain, that more closely approximates the universal physical symmetries. With unification and invariance being connected in this fashion, it is not clear why there is a problem with the notion of the idea of a given invariance group giving an idea of objectivity-at-a-time: our present invariance groups are simply taken to approximate some aspect of objective reality. Similarly, it is not clear why invariantism, so-understood, should be troubled by how the useful symmetries are to be determined. If a theory built upon an invariance group that finds an approximate

invariantism is the attempt "to establish a connection between the condition for objectivity and universality" (Debs and Redhead, 2007, p.60). I take this to mean that invariantism includes the idea that by developing theories of a broader invariance group, we have theories that are more unified and progress towards a universal, theory-of-everything.

263 I am interested in these two because through responding to these we can clarify the sense in which OSR takes objectivity to be the starting-point of scientific analysis.

264 In particular they object to Nozick's argument that the choice of invariance group upon which to define objectivity should be resolved just by "the bootstrap process of scientific investigation" (Nozick, 2000, p.84). For Nozick, then, the proper invariance group to describe that which is objective is determined by the usual scientific inductive procedure according to which new developments emerge out of past theories and lead to the development of new theories. This, Debs and Redhead claim, just asserts the heuristic value of certain symmetries.

265 With respect to the second of Debs and Redhead's objections, French also objects that it is not clear that invariantism is necessarily committed to the idea that the search for objectivity is linked with the search for a unified theory. On one level this seems quite right: there seems to be no reason that one cannot be, e.g., pluralist while insisting that objectivity within each domain is to be determined by that domain's own symmetry groups. However, if we restrict our attention to physics, then it seems perfectly plausible to interpret symmetries as playing a unificatory role. In quantum physics, e.g., elementary particles can be characterised according to their internal symmetry groups: this proved important in the development of quantum physics because the characterisation of hadrons in terms of the SU(3) symmetry group suggested that these particles had key similarities which, ultimately, led to the quark hypothesis. See Falkenberg (1988) and Morrison (2000, ch. 4; 2008).

correlate in the final theory then that theory will be successful; if it is not built upon such an invariance group, on OSR, we would not expect it to enjoy the same success.

On OSR, then, the world is ultimately to be described as a web of relations. This web of relations will contain certain invariants and it is these invariants, understood group-theoretically, that symmetry principles are taken to refer to. Principles and laws relate to each other because laws encode invariance principles in their mathematical form:[266] e.g., Einstein's field equations are objective just because they are invariant under the group *Diff*(*M*). Objects, then, are constituted by these laws. I take it that this much is common to both eliminative and non-eliminative OSR. The web of relations that composes metaphysical reality seems to do all of the explanatory work: i.e., it is the invariants among these web of relations being encoded into our laws that explains the predictive success of our laws. On eliminative OSR, this is understood as meaning that there is no explanatory role for objects and so there is no need to treat them as part of our ontology. From the perspective of non-eliminative OSR, this framework provides a means to understand the primacy of relations over relata—i.e., it is the relations that ultimately explain the success of science—while also maintaining that there must be some relata between which the relations hold.

It should now be clear what it means for both eliminative and non-eliminative OSR to begin the analysis of science with the concept of objectivity. The analysis of science begins with the identification of a particular set of invariants and these symmetry principles are encoded in the laws of a theory. The objects of a theory are then identified as points of intersection between the relations described by laws. The two forms of OSR differ in how they interpret these objects, but I will return to that issue in the next section. For now I wish to emphasise the sense in which this is a different picture from that offered by Cassirer.

From the perspective of my reading of Cassirer's structuralism, the concepts of <objectivity> and <knowledge> have somewhat been run together on OSR. For Cassirer the most fundamental goal of philosophy of science was to explain how it is possible that scientific theories can be objective, while human experience of the world was entirely subjective. Emphasising the foundational importance of particular invariance groups was important because they provide a means to describe frame-dependent experience in frame-independent terms. An essential part of Cassirer's philosophy of science is to show how scientific entities emerge out of the interplay of objectivity, principles and laws. This is a

[266] This feature of OSR is influenced by Cassirer's account of functional coordination. See (Cei and French, forthcoming, p.10): "The sense in which the data, laws and principles relate to each other can be captured via a kind of 'Functional Coordination' (Cassirer, 1956) which is encoded by the mathematical form of the laws and, further, grounds the possibility of scientific objectivity".

much more important feature of Cassirer's philosophy of science than it is for OSR. On OSR science ultimately aims to represent the external world; for Cassirer, science ultimately aims to reveal that which is invariant in every judgment concerning objects.

The upshot of this is that Cassirer and OSR have very different attitudes about the question of the ontological status of the invariance principles that form the basis of particular scientific theories. For Cassirer, invariance principles are ultimately aiming at describing the regulative ideal of the ultimate invariants of experience. For Cassirer it does not even make sense to ask the further question as to whether or not these ultimate invariants of experience correspond with an external reality: they are those elements that are invariant at the root of every human judgment and, as such, cannot be expected to refer to external reality. Indeed, for Cassirer, the study of science was equivalent to the study of human reason or the mind.[267] For Cassirer, if we take science to make claims about the nature of mind-independent reality, that is akin to taking it to make claims about science-independent reality: clearly, then, for Cassirer the additional question that motivates OSR—i.e., as to whether we should explain the success of our science in terms of the reference of symmetry principles to external reality—does not even really make sense.

This, I suggest, is a problem for Cei and French's argument that Cassirer's account of functional coordination can be deployed as an argument for OSR. In running together the concepts of objectivity and knowledge, Cei and French effectively remove Cassirer's separate understanding of objectivity as a regulative ideal. When this is emphasised as part of Cassirer's functional account of coordination, it is clear that Cassirer's Kantianism—and his refusal to make any metaphysical commitments on the basis of his structuralism—is more integral to his structuralism than Cei and French would have it. In the next section I introduce briefly the idea that this distinction is, at root, a distinction between two different accounts of the relationship between philosophy and science. In §5.4, I return to the question of distinguishing Cassirer's account of law-constitutivity from structural realist versions of the claim.

5.3.2. Law-constitutivity and the metaphysics of objects on ontic structural realism

In the previous section I have argued that, as a consequence of his account of objectivity, it does not make sense for Cassirer to ask the question as to whether or not invariance principles refer to any feature of external reality. In this section I wish to clarify that there is a deeper methodological distinction between Kantian and realist approaches that lies

[267] "The 'understanding' here is not to be taken in the empirical sense, as the psychological power of human thought, but arather in the purely transcendental sense, as the whole of intellectual and spiritual culture. It stands directly for that entity which we designate by the name 'science'". (Cassirer, 1981, pp.154-5)

behind this claim to do with the differing accounts of the role of philosophy in science. In particular, on OSR philosophy is seen as a branch of science, whereas for the Kantian philosophy of science is more properly understood as being concerned with epistemology.

From this perspective it is helpful to consider how we might choose between eliminative and non-eliminative OSR. I argue that this is a question that cannot be decided by science itself and, instead, should be understood as a purely metaphysical question. This is because a structuralist account of laws can be used equally well to support either eliminative OSR or non-eliminative OSR: indeed the choice to either include or eliminate objects from one's ontology seems to be independent of the question of whether or not the laws commit us to the existence of such objects. To clarify this, I will consider the relationship between laws and objects given on Cei and French's (forthcoming) eliminative OSR with that of Lyre's (2011) non-eliminative OSR. I argue that, if OSR is to take law-constitutivity as fundamental, then it ought to be cautious about the manner in which it answers the question as to whether or not objects should be eliminated from our ontology, or else be subject to its own version of the argument from metaphysical underdetermination. By this I mean that the laws of our best scientific theories do not yet clearly allow us to say whether or not there are objects: on a realist reading laws refer to structural features of the world, but there is nothing in the laws to say whether or not these structural features are all that there is in the world or whether there are also secondary relata underlying these relationships. This is not to say the realist should not seek to provide an answer to this question; it would be a strange sort of realist who did not attempt to consider the nature of the world that is revealed to us through scientific theories. It is instead to say that there needs to be a clear demarcation between what is revealed to the realist by the nature of the laws of physics—i.e. the primacy of structural features of the world—and what is metaphysical speculation—i.e., how the primacy of structural features of the world should be interpreted.

That OSR is not sufficiently clear that the question of whether or not there are objects cannot be settled by the current best scientific theories, I suggest, is ultimately a consequence of its treating philosophy as a branch of science. That is, the task of philosophy is to try and reveal how the world would be if our current scientific theories are correct. From the Kantian perspective this is a mistaken understanding of the relationship between philosophy and science, and I argue that the most promising Kantian way to resist understanding law-constitutivity in a realist fashion is to insist that philosophy of science be understood epistemologically.

According to non-eliminative OSR at least one of the essential properties of an object must be relational. Lyre defends a version of non-eliminative OSR that is very close to eliminative OSR in that he takes the relata to possess *only* relational properties, where relational properties are understood as including structurally derived intrinsic properties. Structurally derived intrinsic properties are invariants of the structure: e.g., mass and spin are characterised by Casimir operators of the Poincaré group while charge is derived from the U(1)×SU(2) and SU(3) interaction groups.[268] Particles then are "instantiations of the world structure possessing all structurally invariant properties" (2011, p.172).[269]

Lyre's version of non-eliminative OSR provides us with a quite natural conception of structural kinds: i.e., a structural kind of particle is associated with the invariance groups associated with particular values of mass, charge and spin. For Lyre, then, the structure of the world is ontologically primary, as it is on eliminative OSR. Lyre is also committed to treating structural properties—relations and structurally derived invariants—as features of the total structure of the world that individuate kinds of relata. The structural laws, then, since they hold between structural kinds, simply reflect structures in nature.

So for both Cei and French and Lyre, structural laws are simply features of the ontologically basic structure of the world.[270] This, I think, leads to a problem if we seek to approach OSR from a law-constitutive perspective: i.e., a structural account of laws is consistent both with treating the relata that stand in law-like relations as metaphysically real entities and with the understanding the treatment of these relata as objects as merely heuristic. A law-constitutive account of objects on OSR is unable to tell us whether or not the objects that are constituted by the laws of a theory should be understood as substantial metaphysical entities or not.

This concern is similar to that expressed by Brading and Skiles (2012), who argue that OSR suffers from the same type of underdetermination as entity-realism, in that it cannot distinguish between "reductive" or eliminative OSR.[271] Their version of the argument from underdetermination, re-cast in terms of eliminative and non-eliminative OSR is as follows:

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[268] See (Lyre, 2011, p.171).

[269] This approach also has the advantage of enabling us to make sense of what it means for a particle to have zero-value properties, such as mass.

[270] The difference between Cei and French and Lyre is as to whether the structures described by laws are modal or non-modal. For Lyre, taking structurally derived invariants to define structural kinds provides a means to embrace a Humean understanding of laws of nature: i.e., laws are to be understood non-modally and merely as the expression of certain regularities. So, laws like "all electrons have spin ½" is to be explained in terms of the non-modal invariances of the global structure.

[271] Reductive OSR is taken to be the view that objects should be reconceptualised as nodes in a structure, and eliminative OSR is understood as detailed in §5.2, i.e. as the claim that there are only relations and no relata. Brading and Skiles division of OSR into reductive and eliminative OSR is, perhaps, not the most helpful: i.e., I take the claim that objects should be reconceptualised as nodes in a structure to be perfectly compatible with eliminative OSR. Eliminative OSR is perfectly consistent with a heuristic appeal to objects, so long as they are understood as having solely relational properties. The difference between eliminative and non-eliminative OSR is better characterised as being over whether the nodes in a structure have any intrinsic properties.

(P1) If OSR is true, then there is a fact of the matter about whether objects exist or not.

(P2) If ([P1]) is true, then we should expect our best theories to say whether objects exist or not.

(P3) But our best theories fail to say whether objects exist; whether they do or not is underdetermined by the interpretations offered by eliminative and [non-eliminative] OSR.

(C) So OSR is (probably) false. (Brading and Skiles, 2012, p.112)

However, this argument need not trouble OSR. If we take the law-constitutive approach seriously, then this underdetermination can be solved in the same sort of way as Brading and Skiles seek to solve the metaphysical underdetermination that motivates OSR. That is, the proponent of OSR can claim that the laws of our theory are such that they do not tell us anything about the existence or non-existence of objects. All that the structuralist laws tell us is that if the relata that stand in the relations expressed by the laws are to be considered as metaphysical entities, then they should be understood as having only relational properties—perhaps including structurally derived intrinsic properties—and as non-individuals. This account of laws cannot tell us whether or not the relata need to be interpreted as substantial metaphysical entities because nothing in how the success of the laws is understood depends on this point. OSR, then, offers a law-constitutive account of objects only in that it places constraints on how the sense in which the nodes in a structure *can* be considered as objects; it should be silent on whether or not they should be considered as objects. As soon as OSR ceases to be silent about the question of whether or not there really are objects, then it becomes vulnerable to Brading and Skiles's version of the argument from underdetermination.

It may seem strange that a law-constitutive approach to objects does not actually commit the ontic structural realist to either the existence or non-existence of objects. However, if we consider the role of philosophy in OSR, then I think this makes sense. In particular, on OSR[272] philosophy is meant to take science seriously as a guide to our ontology: i.e., taking our best scientific theories as a starting point the task of philosophy is to construct a metaphysical picture of the world that is consistent with our best scientific theories. From the perspective of OSR the most pertinent fact about the laws of our best scientific theories is that they are structural. In the appropriate structural realist metaphysics of the world, then, all that can be said with confidence is that the laws of physics refer to structural features of the world. It is, after all, the structural features of the world that are

[272] This is explicit in (Ladyman and Ross, 2007), but, I take it is equally true of Cei and French's OSR

explanatorily relevant for the success of science. Any claim beyond this must involve the application of characteristically philosophical arguments to science.

Let us consider briefly the type of arguments that may get brought to bear in order to argue for, e.g., non-eliminative over eliminative OSR. The most well-known objection to eliminative OSR is that it is impossible to make sense of relations without relata and, thus, if we are to advocate OSR we ought to support non-eliminative OSR.[273] As French and Ladyman (2011) make clear, there are other metaphysical objections to contend with: for instance part of the debate about whether OSR can make sense of the inherent "activity" of causation.[274] Chakravartty objects that, without relata, it is impossible to make sense of the active function of causality that transforms one set of relations into another. However, eliminative OSR responds to this by treating structures as being modal. If we accept the claim of OSR that laws do refer to features of the world, this only reveals a limited amount: i.e., that it is structures that are explanatorily relevant for the success of science. Going beyond this to endorse either eliminative or non-eliminative OSR requires deployment of metaphysical arguments.

From a Kantian perspective, the problem with OSR ultimately relates to its account of the role of philosophy in science. In effectively treating philosophy as a branch of science whose task is to inquire as to the nature of the world that is revealed to us by science, philosophy exceeds its remit. While, from the last section, we have seen that the Kantian should object to taking invariance principles to refer to external reality in the first place, this understanding of the role of philosophy becomes even more problematic when it seeks to distinguish between eliminative and non-eliminative OSR.

The most fundamental Kantian lesson that should resonate with us today relates to the role of philosophy in science. Specifically Kant cautioned against letting philosophy become a part of science, in the manner that it does on OSR, and instead seems to have understood it as a branch of epistemology. The role of philosophy, on this reading, is not to reveal to directly reveal to us the ontology of the world as described by metaphysics: instead the purpose is to ask what precisely the conditions of the human experience that

[273] On occasion the attitude towards eliminative OSR is outright dismissive, e.g. Wüthrich claims that "Taken at face value, eliminative structural realism…clearly incoherent" (2009, p.1041). The point, from my perspective, is that all that OSR can say without appeal to metaphysical argument is that the best way to understand the success of scientific theories is to understand them as referring to structural features of the world. The scientific realist clearly has a duty to try and explain the primacy of relata over relations; eliminative structural realism is one of two potential explanations of this feature of the world. The argument that eliminative OSR is a viable option must be a metaphysical one: i.e., it must show that a metaphysics consisting solely of relations is coherent and, e.g., appeal to the principle of parsimony to suggest that this is therefore the best explanation of the successive of physical laws. Contra Wüthrich, I think it is clear that substantial steps have been made towards making such an argument that secures the coherence of eliminative OSR, see, e.g. (Ladyman, 2007).

[274] See (Chakravartty, 2003, p.872)

give rise to science are. Cassirer similarly emphasised that the task of philosophy of science was primarily epistemological: i.e., its task was to help us understand science rather than to "dominate" science.[275]

I suggest, then, that this Kantian approach to science is worth emphasising in the contemporary debate. If nothing else, the Kantian understanding of the role of philosophy of science can serve as a prophylactic for OSR, to prevent it straying into the realm of metaphysical underdetermination itself. More generally, though, the Kantian account of the role of philosophy in science is important in its own right. If science is seen as the most characteristically rational human act that provides knowledge of the world—which, I think it is—then, surely, part of the task of philosophy of science must be to explore how the process of science enables us to achieve this knowledge. In this sense it is important to at least have the discussion as to whether invariance principles represent features of external reality or whether they are constructed so as to ensure that science is maximally objective: at the very least this question must be satisfactorily addressed before we can move to ask metaphysical questions about the nature of reality.

5.4. The constitutive role of principles in the development of laws

In the previous section I identified one key difference between structural realist and a neo-Kantian account of laws: for the structural realist the invariance groups that characterise scientific theories represent some feature of metaphysical reality, while on a regulative Kantianism they should be understood as a regulative ideal only. In this section I address the second important distinction. For Cassirer, physical principles are interpreted quite differently to how they are understood on OSR, where the significant principles are symmetry principles and laws of conservation that can be understood in terms of their symmetries. From the perspective of the account of Cassirer developed in §3.3, the version of OSR advocated by Cei and French assigns physical principles the role that Cassirer intended the concept <objectivity>—more properly, <objectivity-at-a-time>—to play. This means that from a Kantian perspective there remains a role for principles that is not captured by OSR. In this section I argue that principles can be understood as playing a constitutive role in a theory in the sense that they make the development of the laws of that theory possible. Cassirer's understanding of the constitutive role of physical principles, I suggest, is the main obstacle that stands in the way of taking Cassirer's law-constitutive account of objects to fairly straightforwardly lead to a form of OSR.

[275] See (Cassirer, 1907, p.31)

There is a distinction between Cassirer's structuralism and OSR in their treatments of the ideal of objectivity. On OSR, the ideal of objectivity is represented by a particular set of symmetry principles which perfectly capture the structure of the world. For Cassirer, the ideal of objectivity is that which is invariant in all judgments concerning experience. This means that Cassirer places a much greater focus on securing the objectivity of the entire sequence of scientific theories than OSR does. This is not to say that OSR does not treat the entire sequence of theories as objective: it clearly does, but on OSR there is a sense in which later theories are *more* objective than earlier theories in that they more accurately represent the end-point of scientific inquiry. For Cassirer, the proper subject of philosophy of science is not individual theories; it is the entire sequence of scientific theories. As the sequence grows then it, as a whole, better approximates objectivity. This is important because I think it underlies the differing attitudes towards physical principles on the two views. So, on my reading of Cassirer physical principles are of central importance because they serve to ensure that new laws continuously emerge from old laws—and in so doing guarantee that the sequence of scientific theories can be treated as a singly object of study. On OSR, laws are of primary concern because the most significant question is as to what our present physical theories reveal of the nature of the world.

Cei and French (2009), then, do not place too much emphasis on this historical role for physical principles, instead—as we have seen—emphasising the importance of symmetry principles and laws. As we saw in §3.3, Cassirer sought to provide an account of the rationality of the entire sequence of scientific theories. For his answer to CR to be plausible he needed to find something that was constant in scientific theorising. Laws are ill-suited to this task because they change from theory to theory. Principles, on the other hand, are taken to be universal claims: while, as we have seen in the case of the equivalence principle, physical principles develop over time and are applied in different ways they are able to serve as a rule to enable the development of laws and secure the rationality of theory change. This is particularly clear in the case of the equivalence principle, which is an insight derived from Newtonian physics that Einstein interpreted regulatively and used to derive new physical laws. When Cassirer's account of the role of physical principles in science is understood in this way, it becomes clear that OSR does not emerge from Cassirer's structuralism as cleanly as Cei and French would have it. Laws are central to Cassirer's philosophy only insofar as they were developed in accordance with physical principles.

If we are to insist that the law-constitutivity of objects can serve as a characteristically *Kantian* answer to CC, then, our account must be accompanied by an insistence on the centrality of physical principles rather than laws. There is some difficulty in cashing this out within a regulative framework that is influenced by Cassirer's neo-Kantianism: Cassirer, in DIMP, is quite clear that the relationship between statements of the results of measurements, of laws and of principles should be understood as non-hierarchical. The different types of physical statements have a "mutual interconnection" and each level conditions and supports both other levels: so there is, for Cassirer, a complex "reciprocal interweaving" between empirical data, laws and principles (p.35).

While acknowledging that there is a deep inter-connectedness among the three elements of physics, Cassirer also claims that "this cannot and must not prevent us…from ascribing to [physics] a determinate structure, a higher and lower order of elements" (*ibid.*). Cassirer is clear that his emphasis on the complex interweaving of the different type of statements is intended only to preclude the possibility of their actual separation, that is, of the assertion of independent existence of, e.g., physical principles. Within Cassirer' system, then, there can be some justification for treating physical principles as being of the highest order and empirical data as the lowest order. In §3.3.2.2, I argued that Cassirer strongly associated objectivity with the idea of permanence: in particular, he argued that the laws of physics are constitutive of the objects of physics because the relations described by the laws are permanent whereas the properties of objects are subject to change. Similarly, there is a sense in which physical principles are in this sense more permanent than the laws of physics because, on Cassirer's account, physical principles can survive scientific theory change and act as the birth-place for laws. So, while physical principles cannot be understood in isolation from laws and empirical data, they are—I would suggest—central to a regulative neo-Kantianism in that principles are the most long-lasting features of physics and determine the content of physical laws.

There is an interesting comparison to be drawn between this understanding of the relationship between laws and principles and that advocated more recently by Lange (2007). Lange argues that physical principles—particularly symmetry principles—are meta-laws: i.e., they are laws that govern and explain laws in precisely the same manner that laws govern and explain facts and events. On Lange's account, for laws and principles to govern events and laws respectively is for them to be as resilient under counterfactual suppositions as they logically possibly can be (p.458). How should this idea of maximal resilience be understood? Consider a conservation law that is associated with a particular regularity. This conservation law can be derived from dynamical laws—such as Newton's laws of motion—force laws and a closure law that states that the only laws are the force laws (p.466). The law is a genuine law if it is the case that in every possible world that the dynamical laws, force laws and closure law are the same as they are in the actual world the conservation law holds. However, this is not enough for maximal resilience under counterfactual suppositions because we can consider a wider range of possible worlds: i.e., those where the dynamical laws and force laws are different from in the actual world. For a conservation law to govern events in Lange's sense, the conservation law must hold even in possible worlds with different force laws. So, similarly, for a symmetry principle to govern a law is to claim that the symmetry principle would have held even if the natural laws had been different.

Lange's approach provides a very clear sense in which principles can be central, *viz.* if the principle holds in possible worlds where the laws do not. This may help to clarify Cassirer's view. For instance, in Cassirer's account of the development of laws he is quite clear that principles come first historically and that the principles guide the composition of the laws. From this perspective it would seem that the content of the laws could have differed while the principles remained the same.[276] Physical principles have a central role in Cassirer's philosophy, then, because they play a historical role in making laws possible: the content of the laws must be consistent with the demands of the physical principles. In §4, I develop an account of the role of the equivalence principle in the development of general relativity that can profitably be understood in these terms. As we have seen, the equivalence principle had its roots in Newtonian physics: there it was used in order to permit the treatment of non-inertial systems as inertial systems. Einstein used this version of the equivalence principle in 1907 to derive equations that predicted that the velocity of light would decrease in a gravitational field. In 1912 Einstein extended the equivalence principle so as to claim the *full* physical equivalence of uniformly accelerating frames of reference and inertial frames of reference equipped with a gravitational field and this move enabled him to develop field equations for static gravitational fields. However, it also forced Einstein to limit the equivalence principle to infinitesimal regions. Nevertheless Einstein remained convinced of the value of the infinitesimal version of the equivalence principle and used it to consider the type of gravitational field that would be associated with uniform rotation: this ultimately led Einstein to associate the equivalence principle with the claim that the metric tensor represents the inertio-gravitational field (EP3).

[276] There are, of course, significant differences between Lange's position and Cassirer's. In particular, Lange's account is within the realist tradition while Cassirer's account is neo-Kantian. The significance of this is that Lange takes our scientific theories—including their laws and principles—to be referring to external reality whereas Cassirer takes the laws of a theory to be developed in such a way that they meet the demands on the one hand of relativized a priori constitutive principles and, on the other hand empirical data: he does not take these theories to refer to the external world. As such, the sort of counterfactuals that must be considered are to do whether we could feasibly have constructed alternative physical laws given the same principles and empirical data, whereas Lange's counterfactuals are concerned with how else the world may have been. This difference, though, does not prevent us, I think, from adapting Lange's criterion for the centrality of principles.

We are also now in a position to make sense of Cassirer's brief comments on the status of the equivalence principle.[277] The field equations of general relativity are frame-independent, however the interpretation of the metric tensor g_{ik}, is frame-dependent: that is it can be interpreted either as an inertial or gravitational effect according to EP3. Janssen (2012) refers to this as the relativity of gravitation: i.e., it is possible for two observers in relative non-inertial motion to disagree about which of them is at rest so long as they agree to disagree about the presence of a gravitational field. The importance of EP3 is just that it secures the relativity of gravitation in this sense and, in so doing, enables the same frame-independent laws to describe quite different frame-dependent experiences. This, I suggest, is how Cassirer understood the sense in which the equivalence principle is constitutive of general relativity: from a given perspective the metric tensor is interpreted either as describing some combination of inertial and gravitational effects and this serves to define a conceptual framework within which experience is to be constructed. In this sense EP3 is constitutive of the field equations just because it makes it possible that they apply equally to physical situations that are perceived of as quite distinct: i.e. accelerating and being at rest in a gravitational field.[278]

Now, this understanding of the constitutive role of the equivalence principle is very close to the account offered by Friedman: i.e., the equivalence principle is understood as a constitutive principle in virtue of its historical role in the development of the gravitational field equations. The distinction between my view and Friedman's lies in the understanding of why constitutive principles are required. On Friedman's account, recall, ostensively defined frames of reference play a role analogous to Kant's faculty of intuition: i.e., our experience of the universe is from a particular perspective associated with a given frame of reference. Within our own frame of reference the basic mechanical concepts such as space, time and motion are coordinated with experience in a particular fashion. The challenge posed by the development of relativity is as to how our frame-based experience can be related to the increasingly abstract mathematical structures of space-time theories. This, for Friedman, is affected by constitutive principles that provide particular ways to coordinate abstract mathematical structures with elements of experience: e.g., EP3 coordinates free-fall trajectories with null geodesics in space-time. These constitutive principles count as synthetic a priori principles for Friedman: they allow us to construct mathematised law-like

[277] Recall the central feature of Cassirer's claim: "The inertial movement and the effect of gravitation, are…in truth a single phenomenon seen and judged from different sides. It follows that the fundamental law that we establish for the movement of bodies must be such that it includes equally the phenomena of inertia and those of gravitation. As is seen, we have here no empirical proposition abstracted from particular observations, but a rule for our construction of physical concepts" (ETR, p.428).

[278] As detailed in §4.3.2.2, this is not an entirely satisfactory account of the constitutive role of EP3: the situation is made much clearer when we have access to affine connected manifolds.

descriptions of the experience given to intuition—understood in terms of ostensively defined frames of reference—in a manner analogous to how Friedman understands Kant to have applied the laws of motion in order to derive Newton's law of gravitation.

Friedman, recall, rejects the possibility of developing a Kantian account of philosophy of science along Cassirer's lines because in rejecting the division between sensibility and understanding Cassirer cannot explain the distinction between constitutive and regulative principles. In §3, I argued that this was not the case, and that Cassirer can distinguish between constitutive and regulative principles. The account of the role of the equivalence principle has served to clarify the distinction between the constitutive and regulative roles of this principle. The equivalence principle played a regulative role in the development of general relativity when Einstein interpreted it as a covariance principle: i.e., in permitting a broader group of coordinate transformations, the equivalence principle provided a closer approximation of the regulative ideal of objectivity than the more limited group of Lorentz transformations. Regulative principles, then, are those that serve to drive theory change towards the regulative ideal of objectivity: they do so either by broadening the invariance group of a theory or by increasing unity. Constitutive principles are those that have played a historical role in making new laws possible by determining a conceptual framework within which the laws can be applied to experience. A particular physical principle can be either:

(i) *Constitutive but not regulative*: Some principles may be constitutive without being regulative: I would suggest that EP3 could be seen as such a principle. This principle is constitutive in the sense that it provides a means to interpret the frame-independent field equations in such a way that they can be interpreted in a frame-dependent fashion. EP3 is not regulative insofar as it does not license a broader class of coordinate transformations.[279]

(ii) *Regulative but not constitutive*: If a regulative principle does not serve to govern the application of concepts to experience, then it does not play a constitutive role. General covariance, as detailed in §4.4, is such a principle. It is regulative in the sense that it broadens the class of admissible coordinate transformations; however it is cannot be constitutive because it has no physical content.

[279] We may want to treat it as regulative in the sense that it does seem to unify gravitational and inertial effects, however. I have decided against doing so on the grounds that for a principle to be properly considered regulative, in the sense that it helps answer CR, its motivation ought to be to provide a better approximation of the regulative ideal. While EP3 does unify gravitational and inertial effects, this was not really the motivation behind the principle, rather it is a consequence of its development out of EP1 and EP2.

(iii) *Regulative and constitutive*: Certain principles would seem to be both regulative and constitutive: e.g., EP1, EP2 and the principle of relativity. These principles are regulative in the sense that they increase the group of admissible coordinate transformations. They both play a constitutive role, though in that that they develop a new conceptual framework within which to interpret new physical laws. The principle of relativity does so because its inconsistency with the light principle is resolved via the relativity of simultaneity. EP1 and EP2 play a constitutive role in that they lead Einstein to the realisation that gravitation should not be understood as a field defined onto space-time but, rather, should be accounted for using space-time structures.

So, the regulative and constitutive roles of physical principles should just be distinguished by the type of role that they have played in the development of a particular theory. This is important because it undermines Friedman's main objection to Cassirer's regulative neo-Kantianism. The regulative approach, then, enables us to make sense of the distinction between constitutive and regulative principles without needing to understand frames of reference as somehow standing in for the Kantian faculty of sensibility and without being committed to the synthetic a priori.

With the role of principles and laws understood in this fashion, we are then able to clarify a Kantian law-constitutive understanding of <objects> in general relativity. In general relativity the field equations are diffeomorphic invariant laws that hold between diffeomorphic variant quantities. The diffeomorphism invariance of the laws is important because it secures the objectivity of our knowledge of the objects between which the laws stand. Without this, there is no way to have objective knowledge of objects in general relativity because there are no objects that are invariant under all possible transformations of the theory. There is a clear sense in general relativity, then, in which mathematical structures are epistemologically prior to objects: objective knowledge of the objects of the theory is not possible independently of the structure of the theory. In particular, then, there is no objective account of how the metric tensor is spread, what is invariant is the relationship between metric and Ricci tensors and the stress energy tensor: observables, such as the line element, can only be known in virtue of this invariant relationship. In this sense the laws of the theory are constitutive of the objects of general relativity: knowledge of observable quantities is only possible in virtue of the laws of the theory.

Friedman's main concern with a regulative Kantianism is that one can no longer make sense of the distinction between regulative and constitutive principles. While it is clear that constitutivity cannot be understood in terms of relativized synthetic a priori principles, I have argued that CC can be answered in two ways:

> CC: Principles can be constitutive of the laws of a theory if they provide a conceptual framework within which to interpret key terms, e.g., g_{ik}, from a particular perspective. Laws are constitutive of the objects of a theory, in the sense that they permit the individuation of these objects.

While my answer to CC does not salvage a notion of constitutivity that is anything like as strong as Kant initially intended, it is, I think, the best that can be done while doing full justice to his insights about the regulative role of reason and it is these insights that prove more helpful in developing a philosophy of science that does justice to the development of space-time theories.

In answer to the problem that motivated this discussion, then, we can now say that the law-constitutive answer to CC is characteristically Kantian insofar as it is accompanied by a suitable understanding of laws. I have given reason, in §4 and this section, to take the laws of general relativity to have been made possible by the equivalence principle. If this is right, then the field-equations do not just straightforwardly reveal the objects of experience on their own. There is some sense in which the conceptual framework provided by the equivalence principle is required in order to physically interpret the field equations and, from there, construct a frame-dependent account of objects.

5.5. Lessons for contemporary philosophy of science

I have now provided answers to the challenges—of the rationality of science and of constitutivity—that I suggested a Kantian philosophy of science must seek to answer. I have argued that an internal account of the rationality of scientific theory change can be offered by emphasising the role that conceptual analysis has played in driving theory change coupled with the regulative demand that later theories be of a broader covariance group than earlier theories. With respect to constitutivity, I have argued that there are two senses in which the idea of constitutivity remains relevant. The main candidate for a constitutive principle that I've considered is the equivalence principle. In §4 I have shown how this principle emerged out of Einstein's dialectical engagement with the classical concept of acceleration. I have shown that EP3 plays an important role in permitting the physical interpretation of the mathematical structures whose natural interpretation had been stripped away by the regulative demands placed upon the theory of relativity. EP3, I have suggested is constitutive in that a new conceptual framework—including the inertio-gravitational field—is required in order to explain the sense in which free-fall trajectories represent affine geodesics. Second, I have argued that physical laws are constitutive of objects in the sense that they permit their individuation.

There may still be a question mark over the sense in which this account is Kantian. As noted in §4 my account of the rationality of the development of science has significant overlap with Post's realist (1971) account and my account of constitutive principles seems quite far removed from Kant's original intention.

First, I suggest that my account of the rationality of science is meaningfully Kantian in the sense that it provides an internal account for the rationality of the development of science. I have argued that general relativity has emerged from a conceptual analysis of an inconsistency between classical physics and special relativity. My answer to CR is intended to show that the success of general relativity can be explained without reference to an external reality whose truth the theory approximates. If the success of general relativity can be explained without reference to an external reality this would significantly lessen the force of the no-miracles argument that underlies most contemporary forms of realism.

Second, it is only to be expected that a contemporary account of constitutive principles differs significantly from Kant's own account. My main disagreement with Friedman is related to his claim that for a contemporary Kantian philosophy of science to be meaningfully Kantian it must salvage the syntheticity of the constitutive a priori. I would suggest that this is to underestimate the significance of how science has changed since Kant. On Kant's understanding of the transcendental approach, the fundamental principles of synthesis are fully determined by the nature of human cognition. This is why, e.g., he understood our idea of a triangle to really be a rule for the construction of a triangle in pure intuition. Similarly, he sought to investigate the conditions for the possibility of Newtonian physics solely by reference to the nature of human cognition. Kant could be successful in developing his account of Newtonian physics because of the peculiarly geometrical methodology that Newton used in the construction of his theory: this enabled Kant to treat the notions of space and time that Newton used as immediately given to us.

However, with the work of Helmholtz it became clear that spatial relationships are not immediately given to us: they are constructed in accordance with rules relating to the displacement of rigid bodies. The development of special relativity reinforced this point: simultaneity was not immediately given: the concept needed to be constructed in accordance with rules of light-signalling. The significance of these developments is that it means that a transcendental analysis of the possibility of science does not solely tell us about the nature of human cognition, instead it informs us about the relationship between the world and our cognition: in particular the type of transcendental analysis I have carried out places great emphasis on those principles that permit us to represent features of the contingent world mathematically.

These constitutive principles cannot be understood to be synthetic principles in Friedman's sense: they are principles, derived from contingent features of the world, that permit the representation of physical processes within a particular conceptual framework. While this is clearly a quite different view from Kant's, I think that it still warrants being considered as a Kantian view. Kant's emphasis was on analysing the possibility of science as it was understood at a particular time: given the changes in science it is to be expected that constitutive principles should identify those features of the world that we exploit in order to represent physical processes. On my account constitutive principles play a vital role in the process by which we construct conceptual frameworks in order to represent physical processes. This, I would suggest is as close to Kant's original insight as we can hope to get given the development of scientific theories since Helmholtz.

The Kantian approach can also be distinguished from a realist approach to philosophy of science by emphasising the difference between the realist's naturalist conception of philosophy of science and the Kantian's epistemological conception. Friedman's *Dynamics of Reason* was, in part intended to salvage a role for philosophy that prevented it being seen as a branch of science itself. The attitude that the task of philosophy is to cash out the metaphysical commitments of our current best scientific theories seems to hold significant sway within contemporary philosophy of science. While I do not think that Friedman is successful in arguing that philosophy should be understood as a meta-paradigm that enables the rationality of theory change, I am in sympathy with his desire to mark out a role for philosophy independently of science.

From the contemporary perspective the important lesson to take from Kant is that the first philosophical question that we should be concerned with is as to the possibility of our natural science as it is understood today (or, as Cassirer understood it, as to the possibility of the entire sequence of scientific theories). When the role of philosophy is understood in this way, there is no reason at all to think that a contemporary Kantianism must concern itself with salvaging the synthetic a priori.

Now, while I have argued that Cassirer's regulative neo-Kantianism provides the most promising route to a contemporary Kantian philosophy of science, I am not in complete agreement with his approach. In particular, I doubt the claim that the study of science was effectively the study of the human faculty of reason, or the "mind". It was this feature of Cassirer's account that made it impossible for him to even consider whether the invariance principles at the root of general relativity and quantum physics might represent some feature of external reality.

I suggest that, if we seek to amend this feature of Cassirer's account, then a Kantian approach to the philosophy of science may be able to commit itself to a form of scientific realism. On some readings of Kant, he took it to at least make sense to *ask*, e.g., whether the world was such that matter was composed of attractive and repulsive force. On this understanding of Kant the importance of the transcendental question is to clarify which aspects of natural science derive from either the human faculties of understanding or reason. Once we have clearly identified those aspects of scientific knowledge that have their source in the nature of the human intellect, we can set about asking what features must come from the external world.

This is the sense in which I see a Kantian approach ultimately being important in contemporary philosophy of science. The important Kantian question is as to the possibility of the objectivity of science. In the case of general relativity I have argued that the equivalence principle and general covariance should be understood as regulative demands: i.e., as demands that originate from the human desire to develop frame-independent laws of nature. On my account, these regulative principles act as methodological demands that guide the development of scientific theories, therefore I suggest that there origin lies primarily in the practice of science. Once we have identified regulative demands in this sense, only then are we in a position to ask whether these regulative demands might *also* represent some feature of external reality.

However, this question cannot be straightforwardly answered. If the Kantian account of the development of general relativity is right, then the success if science can be explained without recourse to something like the no miracles argument. So, we cannot argue simply that the structure of the world must be approximately diffeomorphism invariant so as to explain the success of science. This, I think, is promising because, if the problem can be solved, it points a way to a version of scientific realism that (i) does justice to the Kantian intuition that philosophy of science should be understood as a branch of epistemology and (ii) would provide the foundations of a scientific realism that need not rely on such a notoriously questionable argument as the no miracles argument.

Bibliography

Ainsworth, P. M. (2010). What is Ontic Structural Realism. *Studies in History and Philosophy of Science Part B* 4, 50-57.

Beck, A. (Ed.) (1985-2003). *The Collected Papers of Albert Einstein Volumes 1-10*, translator, Princeton University Press.

Bird, A. (2000). *Thomas Kuhn*. Chesham: Acumen.

Bitbol, M., Kerszberg, P. and Petitot, J. (Eds.) (2009). *Constituting Objectivity: Transcendental Perspectives on Modern Physics*. Berlin: Springer.

Birkhoff, G., and von Neumann, J. (1936): The Logic of Quantum Mechanics. *Annals of Mathematics 37*, 823–843.

Brading, K. (2011). Newton's law-constitutive approach to bodies: a response to Descartes

Brading, K. and Skiles, A. (2012). Underdetermination as a Path to Structural Realism. In E. Landry & D. Rickles (Eds.), *Structural Realism: Structure, Object and Causality* (pp.99-115). Berlin: Springer.

Buchdahl, Gerd (1969). *Metaphysics and the Philosophy of Science: The Classical Origins Descartes to Kant,* Oxford: Basil Blackwell.

Buchwald, J. Z. (1994). *The Creation of Scientific Effects: Heinrich Hertz and Electric Waves*. Chicago: University of Chicago Press.

Carnap, R. (2000). *Logical Syntax of Language* (A. Smeaton, Trans.). London: Routledge. (First published 1934).

Cassirer, E. (1907). Kant und die moderne Mathematik. *Kantstudien* 12, 1-49.

Cassirer, E. (1922). *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit: Erster Band*, 3rd edition. Berlin: Verlag Bruno Cassirer. (First published 1906).

Cassirer, E. (1923). *Substance and Function and Einstein's Theory of Relativity* (W. C. Swabey and M. C. Swabey, Trans.). Chicago: The Open Court Publishing Company. (Substance and Function first published 1910; Einstein's Theory of Relativity first published 1921).

Cassirer, E. (1956). *Determinism and Indeterminism in Modern Physics* (O. T. Benfey, Trans.). New Haven: Yale University Press. (First published 1936).

Cassirer, E. (1957). *The Philosophy of Symbolic Forms Volume 3: The Phenomenology of Knowledge* (R. Manheim, Trans.). New Haven: Yale University Press. (First published 1929).

Cassirer, E. (1969). *The Problem of Knowledge: Philosophy, Science and History Since Hegel* (H. Woglom and C. W. Hendel, Trans.). New Haven and London: Yale University Press. (First published 1946).

Cassirer, E. (1981). *Kant's Life and Thought*. J. Haden (trans.), New Haven and London: Yale University Press. (First published 1918).

Cassirer, E. (2005). Hermann Cohen and the Renewal of Kantian Philosophy (L. Patton, Trans.). *Angelaki: Journal of the Theoretical Humanities vol. 10, no. 1*, pp.95-108. (First published 1912).

Cei, A. and French, S. (2009). On the Transposition of the Substantial into the Functional: Bringing Cassirer's Philosophy of Quantum Mechanics into the Twenty-First Century. In M. Bitbol, P. Kerszberg and J. Petitot (Eds.), *Constituting Objectivity: Transcendental Perspectives on Modern Physics* (pp.95-116), Berlin: Springer.

Cei, A. and French, S. (forthcoming). Getting Away from Governance: A Structuralist Approach to Laws and Symmetries. Available from http://philsci-archive.pitt.edu/id/eprint/5462.

Chakravartty, A. (2003). The structuralist conception of objects. *Philosophy of Science* 70, 867–878.

Cohen, I. B. (1951). The Eternal Search (Book Reviews: The Rise of Scientific Philosophy). *The Scientific Monthly*, Volume 73, Issue 5, 328-329

Debs, T. and Redhead, M. (2007). *Objectivity, Invariance and Convention: Symmetry in Physical Science*. Cambridge, MA: Harvard University Press.

DiSalle, R. (2002a). Reconsidering Kant, Friedman, Logical Positivism, and the Exact Sciences. *Philosophy of Science* 69, 191-211.

DiSalle, R. (2002b). Reconsidering Ernst Mach on Space, Time and Motion. In D. Malament (Ed.), *Reading Natural Philosophy: Essays in the History and Philosophy of Science and Mathematics* (pp.167-191). Chicago and La Salle, IL: Open Court.

DiSalle, R. (2006). *Understanding Space-time: The Philosophical Development of Physics from Newton to Einstein*, Cambridge: Cambridge University Press.

DiSalle, R. (2010). Synthesis, the Synthetic A Priori, and the Origins of Modern Space-Time Theory. In M. Domski and M. Dickson (Eds.), *Discourse on a New Method: Reinvigorating the Marriage of History and Philosophy of Science* (pp.523-551). Chicago and La Salle, IL: Open Court.

Domski, M. and Dickson, M. (Eds.) (2010). *Discourse on a New Method: Reinvigorating the Marriage of History and Philosophy of Science*. Chicago and La Salle, IL: Open Court.

Earman, J. and Norton, J. D. (1987). What Price Spacetime Substantivalism? The Hole Story. *British Journal of Philosophy of Science* 38, 515-525

Einstein, A. (1905). On the Electrodynamics of Moving Bodies. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 2: The Swiss Years: Writings, 1900-1909 (English translation supplement)* (pp.140-171). Princeton: Princeton University Press (1989).

Einstein, A. (1907). On the Relativity Principle and the Conclusions Drawn from It. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 2: The Swiss Years: Writings, 1900-1909 (English translation supplement)* (pp.252-311). Princeton: Princeton University Press (1989).

Einstein, A.(1911). On the Influence of Gravitation on the Propagation of Light. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 3: The Swiss Years: Writings, 1909-1911 (English translation supplement)* (pp.379-87). Princeton: Princeton University Press (1993).

Einstein, A. (1912a). The Speed of Light and the Statics of the Gravitational Field. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.95-106). Princeton: Princeton University Press (1996).

Einstein, A. (1912b). On the Theory of the Static Gravitational Field. In A. Beck (Ed. & Trans.), *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.107-20). Princeton: Princeton University Press (1996).

Einstein, A. (1912c). Relativity and Gravitation: Reply to a Comment by M. Abraham. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.130-4). Princeton: Princeton University Press (1996).

Einstein, A. (1913a). Outline of a Generalized Theory of Relativity and of a Theory of Gravitation: Physical Part. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.151-71). Princeton: Princeton University Press (1996).

Einstein, A. (1913b). Theory of Gravitation. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.190-1). Princeton: Princeton University Press (1996).

Einstein, A. (1913c). On the Present State of the Problem of Gravitation. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.198-222). Princeton: Princeton University Press (1996).

Einstein, A. (1914a). Physical Foundations of a Theory of Gravitation. . In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.192-7). Princeton: Princeton University Press (1996).

Einstein, A. (1914b). On the Theory of Gravitation. . In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.291-2). Princeton: Princeton University Press (1996).

Einstein, A. (1914c). On the Relativity Problem. In A. Beck (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 4: The Swiss Years: Writings, 1912-1914 (English translation supplement)* (pp.306-14). Princeton: Princeton University Press (1996).

Einstein, A. (1914d). The Formal Foundation of the General Theory of Relativity. In A. Engel (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 6: The Berlin Years: Writings, 1914-1917 (English translation of selected texts)* (pp.30-84). Princeton: Princeton University Press (1997).

Einstein, A. (1915). On the General Theory of Relativity. In A. Engel (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 6: The Berlin Years: Writings, 1914-1917 (English translation of selected texts)* (pp.98-107). Princeton: Princeton University Press (1997).

Einstein, A. (1916). The Foundation of the General Theory of Relativity. In A. Engel (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 6: The Berlin Years: Writings, 1914-1917 (English translation of selected texts)* (pp.146-200). Princeton: Princeton University Press (1997).

Einstein, A. (1918). On the Foundations of the General Theory of Relativity, In A. Engel (Ed. & Trans.) *The Collected Papers of Albert Einstein, Volume 7: The Berlin Years: Writings, 1918-1921 (English translation of selected texts)* (pp.33-5). Princeton: Princeton University Press (2002).

Einstein, A. (1919). Time, Space, and Gravitation. *Times* (London), 28th Nov 1919, 13-14.

Einstein, A. (1922). Geometry and Experience (G. B. Jeffery and W. Perret, Trans.). In A. Einstein (Ed.), *Sidelights on Relativity* (pp.24-56). London: Methuen & Co., Ltd.

Einstein, A. (1925). Nichteuklidische Geometrie und Physik. *Die neue Rundschau* 36, 16-20.

Einstein, A. (1996). *Autobiographical Notes* (P. A. Schlipp Ed. and Trans.). Chicago & La Salle, Illinois: Open Court. (First published 1949)

Einstein, A. (1954). Notes on the Origin of the General Theory of Relativity. In S. Bargmann (Trans.) *Ideas and Opinions* (pp.285-90). New York: Crown. (First published 1933).

Esfeld, M. (2009). The Modal Nature of Structures in Ontic Structural Realism *International Studies in the Philosophy of Science* 23, 179-194.

Falkenberg, B. (1988). The Unifying Role of Symmetry Principles in Particle Physics. *Ratio* 1, 113-134.

Ferrari, M. (2012). Between Cassirer and Kuhn. Some remarks on Friedman's relativized a priori. *Studies in History and Philosophy of Science* 43, 18–26.

Frank, P. (1951). *Relativity: A Richer Truth*.

French, S. (1989) 'Identity and Individuality in Classical and Quantum Physics', *Australasian Journal of Philosophy* **67,** 432–446.

French, S. (2011). Metaphysical underdetermination: Why worry? *Synthese* 180, 205–221.

French, S. (2012). The Presentation of Objects and the Representation of Structure. In E. Landry and D. Rickles (Eds.), *Structural Realism: Structure, Object and Causality* (pp.3-28). Berlin: Springer.

French, S. and Ladyman, J. (2003). Remodelling structural realism: quantum physics and the metaphysics of structure. *Synthese* 136, 31–56

French, S. and Ladyman, J. (2011). In Defence of Ontic Structural Realism. In P. Bokulich & A. Bokulich (Eds.), *Scientific Structuralism* (pp.25-42). Berlin: Springer.

Friedman, M. (1983). *Foundations of Space-Time Theories: Relativistic Physics and Philosophy of Science*. Princeton, NJ: Princeton University Press.

Friedman, Michael (1991). Regulative and Constitutive. *Southern Journal of Philosophy*, Vol. XXX (Supplement), pp.73-102.

Friedman, M. (1992a). *Kant and the Exact Sciences*. Cambridge, MA: Harvard University Press.

Friedman, M. (1992b). Causal laws and the foundations of natural science. In P. Guyer (Ed.), *The Cambridge Companion to Kant* (pp.161-199). Cambridge: Cambridge University Press.

Friedman, M. (1999). *Reconsidering Logical Positivism*. Cambridge: Cambridge University Press.

Friedman, M. (2000). *A Parting of the Ways: Carnap, Cassirer and Heidegger*. Chicago: Open Court.

Friedman, M. (2001). *Dynamics of Reason: The 1999 Kant Lectures at Stanford University*. Stanford, CA: CSLI Publications.

Friedman, M. (2002). Geometry as a Branch of Physics: Background and Context for Einstein's "Geometry and Experience". In D. Malament (Ed.), *Reading Natural Philosophy: Essays in the History and Philosophy of Science and Mathematics* (pp.193-229). Chicago and La Salle, IL: Open Court.

Friedman, M. (2005). 'Ernst Cassirer and Contemporary Philosophy of Science', *Angelaki: Journal of the Theoretical Humanities vol. 10, no. 1*, pp.119-28.

Friedman, M. (2008). Einstein, Kant, and the A Priori. In M. Massimi (Ed.), *Kant and the Philosophy of Science Today: Royal Institute of Philosophy Supplement 63* (pp.95-112). Cambridge: Cambridge University Press.

Friedman, M. (2009). Newton and Kant on absolute space: from theology to transcendental philosophy. In M. Bitbol, P. Kerszberg and J. Petitot (Eds.), *Constituting Objectivity: Transcendental Perspectives on Modern Physics* (pp.35-50). Berlin: Springer.

Friedman, M. (2010a). 'Synthetic History Reconsidered'. In M. Domski & M. Dickson (Eds.) *Discourse on a New Method: Reinvigorating the Marriage of History and Philosophy of Science* (pp.569-813). Chicago & La Salle, Illinois: Open Court

Friedman, M. (2010b). 'Ernst Cassirer and Thomas Kuhn: The Neo-Kantian Tradition in the History and Philosophy of Science', in R.A. Makkreel and S. Luft (eds.) *Neo-Kantianism in Contemporary Philosophy*, Bloomington and Indianapolis: Indiana University Press.

Friedman, M. (2012). Reconsidering the dynamics of reason: Response to Ferrari, Mormann, Nordman. *Studies in the History and Philosophy of Science* 43, 47-53.

Friedman, M. (2013). *Kant's Construction of Nature: A Reading of the Metaphysical Foundations of Natural Science*. Cambridge: Cambridge University Press.

Ghins, M. & Budden, T. (2001). The Principle of Equivalence. *Studies in the History and Philosophy of Modern Science Part B: Studies in the History and Philosophy of Modern Physics*, Vol. 32, No. 1, 33-51.

Gower, B. (2000). Cassirer, Schlick And 'Structural' Realism: The Philosophy Of The Exact Sciences In The Background To Early Logical Empiricism. *British Journal for the History of Philosophy* 8 (1),71-106.

Gray, J. (1979). *Ideas of Space*, Oxford: OUP

Gray, J. (2007). *Worlds out of Nothing*: *A Course in the History of Geometry in the 19th Century*, Springer.

Guyer, P. (1987). *Kant and the Claims of Knowledge*, Cambridge: Cambridge University Press.

Guyer, P. (1990). Reason and Reflective Judgment: Kant on the Significance of Systematicity. *Noûs* 24, 17-43

Hatfield, G. (1990). *The Natural and the Normative: Theories of Spatial Perception from Kant to Helmholtz*, Cambridge, Mass.: The MIT Press.

Heidelberger, M. (1998). From Helmholtz's Philosophy of Science to Hertz's Picture Theory. In D. Baird, R.I.G. Hughes and A. Nordmann (eds.) *Heinrich Hertz: Classical Physicist, Modern Philosopher*, London: Kluwer Academic Publishers.

Heis, J. (2010). "Critical Philosophy begins at the very point where logistic leaves off": Cassirer's response to Frege and Russell. *Perspectives on Science,* vol. 18, no. 4, 383-408.

Heis, J. (2011a). Ernst Cassirer's Neo-Kantian Philosophy of Geometry. *British Journal for the History of Philosophy* 19, 759-794.

Heis, J. (2011b). Ernst Cassirer's Substanzbegriff und Funktionsbegriff. Unpublished manuscript, forthcoming in *HOPOS: The Journal of the International Society for the History of Philosophy of Science*. Retrieved from http://www.lps.uci.edu/lps_bios/jheis.

Heis, J. (2012). Realism, Functions, and the A Priori: Ernst Cassirer's Philosophy of Science. Unpublished manuscript. Retrieved from http://www.lps.uci.edu/ lps_bios/jheis.

Helmholtz, H. (1878). 'The Origin and Meaning of Geometrical Axioms II', *Mind*, Vol.3 No.10, 212-225.

Helmholtz, H. (1903). *Vorträge und reden von Hermann von Helmholtz*, 5th edition. Braunschweig, F. Vieweg und sohn.

Helmholtz, H. (1977a). On the Origin and Significance of the Axioms of Geometry. In R.S. Cohen and M.W. Elkana (Eds.) and M.F. Lowe (Trans.), *Hermann von Helmholtz: Epistemological Writings* (pp.1-38). Boston-USA: D. Reidel Publishing Company. (First published 1868)

Helmholtz, H. (1977b). The Facts in Perception. In R.S. Cohen and M.W. Elkana (eds.) and M.F. Lowe (trans.) *Hermann von Helmholtz: Epistemological Writings* (pp.115-185). Boston-USA: D. Reidel Publishing Company. (First published 1878)

Lenoir, Timothy (2006). 'Operationalizing Kant: Manifolds, Models and Mayhematics in Helmholtz's Theory of Perception', in M. Friedman and A. Nordmann (eds.) *The Kantian Legacy in Ninetieenth Century Science*. Cambridge, Mass.: The MIT Press.

Hertz, H. (1891). 'Hermann von Helmholtz', in D. E. Jones and G. A. Schott (trans.) *Miscellaneous Papers*. London: MacMillan.

Hertz, H. (1893). *Electric waves: being researches on the propagation of electric action with finite velocity through space*, D.E. Jones (trans.), London: MacMillan and co.

Hertz, H. (2007). *The Principles of Mechanics Presented in a New Form*, D.E. Jones and J.T. Walley (trans.), New York: Cosimo Classics (First published 1899).

Howard, D. (2005). Albert Einstein as a Philosopher of Science. *Physics Today*, Dec., 34-40.

Howard, D. (2010). "Let me briefly indicate why I do not find this standpoint natural." Einstein, General Relativity, and the Contingent A Priori. In M. Domski & M. Dickson (Eds.), *Discourse on a New Method: Reinvigorating the Marriage of History and Philosophy of Science* (pp.333-355). Chicago & La Salle, Illinois: Open Court.

Ihmig, K-N. (1999). Ernst Cassirer and the Structural Conception of Objects in Modern Science: The Importance of the "Erlanger Programm". *Science in Context* 12, 513-529.

Janssen, M. (2005). Of pots and holes: Einstein's bumpy road to general relativity. *Annalen der Physik* 14 (Supplement), 58-85.

Janssen, M. (2012). The twins and the bucket: How Einstein made gravity rather than motion relative in general relativity. *Studies in History and Philosophy of Modern Physics* 43, 159-175.

Janssen, M., Norton, J. D., Renn, J., Sauer, T. and Stachel, J. (Eds.) (2007). *The Genesis of General Relativity* (4 volumes). Dordrecht: Springer.

Janssen, M., Renn, J., Sauer, T., Norton, J. D. and Stachel, J. (2007). A Commentary on the Notes on Gravity in the Zurich Notebook. In M. Janssen, J.D. Norton, J. Renn, T. Sauer and J. Stachel (Eds.), *The Genesis of General Relativity Volume 2 Einstein's Zurich Notebook: Commentary and Essays* (pp.489-714). Dordrecht: Springer

Kaiser, Walter (1993). 'Helmholtz's Instrumental Role in the Formation of Classical Electrodynamics', in D. Cahan (ed.) *Hermann Helmholtz and the Foundations of Nineteenth Century Science*, Berkeley: University of California Press.

Kant, I. (1965). *Critique of Pure Reason* (N. Kemp-Smith, Trans.). New York: St. Martin's Press. (First published 1787).

Kant, I. (1992). On the Form and Principles of the Sensible and the Intelligible World [Inaugural Dissertation]. In R. Meerbote and D. Walford (Eds. and Trans.) *Kant's Theoretical Philosophy 1755-1780* (pp.373-416). Cambridge: Cambridge University Press. (First published 1770)

Kant, I. (2004). *Metaphysical Foundations of Natural Science* (M. Friedman, Trans.). Cambridge Texts in the History of Philosophy, Cambridge: Cambridge University Press (First published 1786)

Kant, I. (2007). *Critique of Judgment*, (J. C. Meredith, Trans.). Oxford: Oxford University Press. (First published 1790)

Kant, I. (2010). *Prolegomena to Any Future Metaphysics*, (G. Hatfield, Trans.) (Revised Edition). Cambridge Texts in the History of Philosophy, Cambridge: Cambridge University Press (First published 1783).

Kuhn, T. (1996). *The Structure of Scientific Revolutions* (3rd ed.). Chicago: The University of Chicago Press. (First published 1962).

Ladyman, J. (1998). What is Structural Realism? *Studies in the History and Philosophy of Science*, Vol. 29, No. 3, 409-424.

Ladyman, J. (2007). On the Identity and Diversity of Objects in a Structure. *Proceedings of the Aristotelian Society Supplementary Volume 2,* LXXXI, 23-43.

Ladyman, J. (2009). Structural Realism. *Stanford Encyclopaedia of Philosophy*. http://plato.stanford. edu/entries/structural-realism/#KanESR

Ladyman, J. and Ross, D. (2007). *Every Thing Must Go: Metaphysics Naturalized*. Oxford: Oxford University Press.

Lange, M. (2007). Laws and meta-laws of nature: Conservation laws and symmetries. *Studies in History and Philosophy of Modern Physics* 38, 457-481.

Langton, R. (1998). *Kantian Humility: Our Ignorance of Things in Themselves*. Oxford: Oxford University Press.

Lenoir, T. (1993). 'The Eye as Mathematician: Clinical Practice, Instrumentation, and Helmholtz's Construction of an Empiricist Theory of Vision', in D. Cahan (ed.) *Hermann Helmholtz and the Foundations of Nineteenth Century Science*, Berkeley: University of California Press.

Lenoir, T. (2006). 'Operationalizing Kant: Manifolds, Models and Mathematics in Helmholtz's Theory of Perception', in M. Friedman and A. Nordmann (eds.) *The Kantian Legacy in Nineteenth Century Science*. Cambridge, Mass.: The MIT Press.

Lorentz, H. (Ed.) (1952). *The Principle of Relativity* (W. Perrett & G. B. Jeffery, Trans.). New York: Dover Publications. (First published 1923).

Lyre, H. (2011). Structural Invariants, Structural Kinds, Structural Laws. In D. Dieks, W. J. Gonzalez, S. Hartmann, M. Stöltzner & M. Weber (Eds.), *Probabilities, Laws, and Structures* (pp.169=181). Dordrecht: Springer.

Massimi, M. (2011). Structural Realism: A Neo-Kantian Perspective. In P. Bokulich & A. Bokulich (Eds.), *Scientific Structuralism* (pp.1-23). Berlin: Springer.

McArthur, D. (2008). Theory Change, Structural Realism and the Relativized *A Priori*. *International Studies in the Philosophy of Science*, Vol. 22, No. 1, 5-20.

Miller, A. I. (1980). *Albert Einstein's Special Theory of Relativity: Discovery (1905) and Early Interpretation (1905-12)*. Boston, MA: Addison-Wesley.

Mittelstaedt, P. (2009). The Constitution of Objects in Classical Physics and in Quantum Physics. In M. Bitbol, P. Kerszberg and J. Petitot (Eds.), *Constituting Objectivity: Transcendental Perspectives on Modern Physics* (pp.169-181), Berlin: Springer.

Morrison, M. (2000). *Unifying Scientific Theories: Physical Concepts and Mathematical Structures*. Cambridge: Cambridge University Press.

Newton, I. (1995). *Philosophiæ Naturalis Principia Mathematica* (3rd ed.)(A. Motte, Trans.). New York: Prometheus Books. (First published 1729)

Norton, J. D. (1989a). What was Einstein's Principle of Equivalence? In D. Howard & J. Stachel (Eds.), *Einstein and the History of General Relativity* (pp.5-47). Boston: Birkhäuser. (First published 1985)

Norton, J. D. (1989b). Coordinates and Covariance: Einstein's view of space-time and the modern view. *Foundations of Physics* 19, 1215-1263.

Norton, J. D. (1995). Eliminative Induction as a Method of Discovery: How Einstein Discovered General Relativity. In J. Leplin (Ed.) *The Creation of Ideas in Physics* (pp.29-69). Dordrecht: Kluwer Academic Publishers.

Norton, J. D. (2003). General Covariance, Gauge Theories, and the Kretschmann Objection. In K. Brading and E. Castellani (Eds.), *Symmetries in Physics: Philosophical Reflections* (pp.110-123). Cambridge: Cambridge University Press.

Norton, J. D. (2005). A conjecture on Einstein, the Independent Reality of Spacetime Coordinate Systems and the Disaster of 1913. In A. J. Kox and J. Eisenstaedt (Eds.), *The Universe of General Relativity* (pp.67-102). Boston: Birkhaeuser.

Norton, J. D. (2007a). What Was Einstein's "Fateful Prejudice"? In M. Janssen, J.D. Norton, J. Renn, T. Sauer, J. Stachel and L. Divarci (Eds.), *The Genesis of General Relativity Volume 2 Einstein's Zurich Notebook: Commentary and Essays* (pp.715-784). Dordrecht: Springer

Norton, J. D. (2007b). Einstein, Nordstrom, and the Early Demise of Scalar, Lorentz Covariant Theories of Gravitation. In J. Renn and M. Schemmel (Eds.) *The Genesis of General Relativity Volume 3. Gravitation in the Twilight of Classical Physics: Between Mechanics, Field Theory, and Astronomy* (pp.413-488). Dordrecht: Springer.

Okruhlik, K. (1986). Kant on Realism and Methodology. In R.E. Butts (Ed.), *Kant's Philosophy of Natural Science* (pp.307-329). Dordrecht: D. Reidel Publishing Company

Okon, E. & Callender, C. (2011). Does Quantum Mechanics Clash with the Equivalence Principle—and Does It Matter? *European Journal for Philosophy of Science 1*, 133-145.

Padovani, F. (2011). Relativizing the relativized a priori: Reichenbach's axioms of coordination divided. *Synthese* 181, 41-62.

Pasch, M. (1882). *Vorlesungen über neuere Geometrie.* Berlin: Springer. Electronic version retrieved from http://archive.org/details/vorlesungenbern00pascgoog.

Patton, L. (2005). 'The Critical Philosophy Renewed: the bridge between Hermann Cohen's early work on Kant and later philosophy of science', *Angelaki: Journal of the Theoretical Humanities vol. 10, no. 1* pp.109-118.

Patton, L. (2009). 'Signs, toy models and the a priori: from Helmholtz to Wittgenstein', *Studies in the History and Philosophy of Science* 40, pp.281-289

Poma, A. (1997). *Critical Philosophy of Hermann Cohen*, New York: State University of New York Press.

Poincaré, H. (1905). *Science and Hypothesis* (W. J. Greenstreet, Trans.). London and Newcastle-on-Tyne: The Walter Scott Publishing Co., Ltd. (First published 1902).

Pooley, O. (2002). *The Reality of Spacetime.* Doctoral thesis, defended at Oxford University.

Post, H. R. (1971). Correspondence, Invariance and Heuristics: In Praise of Conservative Induction. *Studies in the History and Philosophy of Science* 2, 213-255.

Reichenbach, H. (1965). *The Theory of Relativity and A Priori Knowledge* (M. Reichenbach, Trans.). Berkeley and Los Angeles, CA: University of California Press. (First Published 1920).

Reichenbach, H. (1973). *The Rise of Scientific Philosophy*. Berkeley and Los Angeles: University of California Press. (First published 1951).

Renn, J. (2007a). Classical Physics in Disarray: The emergence of the Riddle of Gravitation. In M. Janssen, J.D. Norton, J. Renn, T. Sauer, J. Stachel and L. Divarci (Eds.), *The Genesis of General Relativity Volume 1*(pp.21-80). Dordrecht: Springer.

Renn, J. (2007b). The Summit Almost Scaled: Max Abraham as a Pioneer of a Relativistic Theory of Gravitation. In J. Renn and M. Schemmel (Eds.), *The Genesis of General Relativity Volume 3. Gravitation in the Twilight of Classical Physics: Between Mechanics, Field Theory, and Astronomy* (pp.305-30). Dordrecht: Springer.

Richardson, A. (1998). *Carnap's construction of the world: The Aufbau and the emergence of logical empiricism*. Cambridge: Cambridge University Press.

Richardson, A. (2007). "That Sort of Everyday Image of Logical Positivism": Thomas Kuhn and the Decline of Logical Empiricist Philosophy of Science. In A. Richardson and T. Uebel (Eds.), *The Cambridge Companion to Logical Empiricism* (pp.346-369). Cambridge: Cambridge University Press.

Rosenberg, J. F. (2005). *Accessing Kant*, Oxford: Oxford University Press.

Ryckman, T. (1991). *Conditio Sine Qua Non*? *Zuordnung* in the Early Epistemologies of Cassirer and Schlick. *Synthese* 88, 57-95.

Ryckman, T. (1999). Einstein, Cassirer and General Covariance — Then and Now. *Science in Context* 12, pp.585-619.

Ryckman, T. (2005). *The Reign of Relativity*. Oxford: OUP.

Ryckman, T. (2008). Invariance Principles as Regulative Ideals: From Wigner to Hilbert. In M. Massimi (Ed.), *Kant and Philosophy of Science Today* (pp.63-80). Cambridge: Cambridge University Press.

Ryckman, T. (2010). The "Relativized A Priori": An Appreciation and a Critique. In M. Domski & M. Dickson (Eds.) *Discourse on a New Method: Reinvigorating the Marriage of History and Philosophy of Science* (pp.455-470). Chicago & La Salle, Illinois: Open Court

Rynasiewicz, R. (2005). Weyl vs. Reichenbach on Lichtgeometrie. In A. J. Kox and J. Eisenstaedt (Eds.)*, The Universe of General Relativity* (pp.137-156).Boston: Birkhäuser.

Saunders, S. (2013). Rethinking Newton's *Principia*. *Philosophy of Science* 80: 22-48

Schlick, M. (1978a). The nature of Truth in Modern Logic (P.Heath, Trans.). In H. L. Mulder & B. van de Velde-Schlick (Eds.), *Moritz Schlick: Philosophical Papers, Volume I*

(1909-1922) (pp.41-103). Dordrecht: D. Reidel Publishing Company. (First published 1910).

Schlick, M. (1978b). Critical or Empiricist Interpretation of Modern Physics? Remarks on Ernst Cassirer's Einstein's Theory of Relativity (P. Heath, Trans.). In H. L. Mulder & B. van de Velde-Schlick (Eds.), *Moritz Schlick: Philosophical Papers, Volume I (1909-1922)* (p.322-334). Dordrecht: D. Reidel Publishing Company. (First published 1921).

Schlick, M. (1985). *General Theory of Knowledge* (H. Feigl and A. E. Blumberg, Trans.). Peru, IL: Open Court.

Skidelsky, E. (2008). *Ernst Cassirer: The Last Philosopher of Culture*, Oxford: Princeton University Press.

Smolin, L. (2005). The Case for Background Independence. In D. Rickles, S. French and, J. Saatsi (Eds.), *The Structural Foundations of Quantum Gravity* (pp.196-239). Oxford: Oxford University Press.

Stachel, J. (1989a). The Rigidly Rotating Disk as the "Missing Link" in the History of General Relativity. In D. Howard & J. Stachel (Eds.), *Einstein and the History of General Relativity* (pp.48-62). Boston: Birkhäuser. (First published 1980).

Stachel, J. (1989b). Einstein's Search for General Covariance, 1912-1915. In D. Howard & J. Stachel (Eds.), *Einstein and the History of General Relativity* (pp.63-100). Boston: Birkhäuser. (First published 1980).

Stachel, J. (2006). Structure, Individuality, and Quantum Gravity. In D. Rickles, S. French and J. Saatsi (Eds.), *The Structural Foundations of Quantum* Gravity (pp.53-82). Oxford: Oxford University Press.

Stachel, J. (2007a). The First Two Acts. In M. Janssen, J.D. Norton, J. Renn, T. Sauer, J. Stachel and L. Divarci (Eds.), *The Genesis of General Relativity Volume 1, Einstein's Zurich Notebook: Introduction and Source* (pp.81-111). Dordrecht: Springer.

Stachel, J. (2007b). The Story of Newstein or: Is gravity just another pretty force? In J. Renn and M. Schemmel (Eds.), *The Genesis of General Relativity Volume 4. The Promise of Mathematics* (pp.1041-1078). Dordrecht: Springer.

Uebel, T. (2012). De-Synthesising the relative a priori. *Studies in the History and Philosophy of Science* 43, 7-17.

Van Fraassen, B. C. (2008). *Scientific Representation: Paradoxes of Perspective*. Oxford: Oxford University Press.

Warren, D. (2001). *Reality and Impenetrability in Kant's Philosophy of Nature*. New York: Routledge.

Warren, D. (2010). Kant on Attractive and Repulsive Force: The Balancing Argument. In M. Domski & M. Dickson (Eds.), *Discourse on a New Method: Reinvigorating the Marriage of History and Philosophy of Science* (pp.193-241). Chicago & La Salle, Illinois: Open Court.

Watkins, E. (1997). The Laws of Motion from Newton to Kant. *Perspectives on Science* vol. 5, 311-348.

Watkins, E. (2005). *Kant and the Metaphysics of Causality*, Cambridge: Cambridge University Press.

Weyl, H. (1921). On infinitesimal geometry: relationship with projective and conformal concepts (Trans. D.H. Delphenich). Available online at http://www.neo-classical-physics.info/ mobile /topology-and-geometry.html.

Weyl, H. (1952). *Space-time-matter* (4th ed.) (Trans. H. L. Brose). New York: Dover Publications. (First published 1922).

Weyl, H. (1982). *Symmetry*. Princeton: Princeton University Press. (First published 1952)

Worrall, J. (1989). Structural Realism: The Best of Both Worlds? *Dialectica* 43, 99-124.

Wüthrich, C. (2009). Challenging the Spacetime Structruralist. *Philosophy of Science* 76, 1039-1051.